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The Introduction of Mesmerism, as an Anæsthetic and Curative Agent into the Hospitals of India. By James Esdaile, M.D., late Presiding Surgeon, Calcutta; Author of Mesmerism in India. Dedicated to the Medical Profession. Pesth, 1852. 8vo. Pp. 46.

Chapters on Mental Physiology. By Henry Holland, M.D., F.R.S., &c. &c., Fellow of the Royal College of Physicians, and Physician in Ordinary to His Royal Highness Prince Albert. Founded chiefly on Chapters contained in Medical Notes and Reflections, by the same Author. London, 1852. 8vo. Pp. 301.

Class-Book of Botany; being an Introduction to the Study of the Vegetable Kingdom. By J. H. Balfour, M.D., F.R.S.E., Regius Keeper of the Royal Botanic Garden, Professor of Medicine and Botany in the University of Edinburgh, &c. &c., with upwards of 1000 Illustrations. Edinburgh, 1852. 8vo. Pp. 357. Part I.

The Pathology and Treatment of Stricture of the Urethra. By John Harrison, Fellow of the Royal College of Surgeons of England, and formerly Lecturer on Surgical Anatomy. London, 1852. 8vo. Pp. 104.

Urologie. Sur les Avantages des Bougies Fortillées et Crochues, Dans les Retreussemens et Augusties de l'Urettre, Difficiles a francher. Par Le Dr Leroy D'Etiolles, Membre Honorairè de l'Academiè des Sciences de Munich, &c. &c. A Paris, 1852. 8vo. Pp. 32.

Hydropathy, as applied to Acute Disease. Illustrated by Cases. By T. R. Armitage, M.B. Lond., M.R.C.S. London, 1852. Post 8vo. Pp. 178.

Pathological Anatomy, considered in relation to Medical Science; being an Address delivered at the Royal Cork Institution, Introductory to a Course on Pathological Anatomy and Histology. By Thomas S. Holland, M.D., M.R.C.I.L., &c. Cork, 1852. 8vo. Pp. 31.

Statistical Report of the Epidemic Cholera in Jamaica. By John Parkin, M.D., &c., Fellow of the Royal Medical and Chirurgical Society, London, &c. &c. &c. London, 1852. 8vo. Pp. 61.

Homœopathy Unveiled: or, Observations on Hahnemann, his Doctrines, and Treatment of Disease. By William Henry Brodribb, Member of the Royal College of Surgeons, England, &c. Second Edition. London, 1852. 8vo. Pp. 45.

General Board of Health. Second Report on Quarantine. Yellow Fever. Presented to both Houses of Parliament, by command of Her Majesty. London, 1852. 8vo. Pp. 414.

On the Diseases of the Bladder and Prostate Gland. By William Coulson, Surgeon to St Mary's Hospital, &c. Fourth Edition, Revised and Enlarged. London, 1852. 8vo. Pp. 485.

On Diseases of the Liver. By George Budd, M.D., F.R.S., Professor of Medicine in King's College, London, and Fellow of Caius College, Cambridge. Second Edition. London, 1852. 8vo. Pp. 486.

Insanity: its Causes, Prevention, and Cure; including Apoplexy and Epilepsy, and Congestion of the Brain. By Joseph Williams, M.D. Second Edition. London, 1852. Post 8vo. Pp. 317.

Magic, Witchcraft, Animal Magnetism, Hypnotism, and Electro-Biology. By James Braid, M.R.C.S., Edinburgh; C.M.W.S. Third Edition, greatly Enlarged. London, 1852. 12mo. Pp. 122.

Homœopathy and the Homœopaths. By T. Stevenson Bushnan, M.D., Fellow of the Royal College of Physicians, Edinburgh; Physician to the Metropolitan Free Hospital, &c. &c. London, 1852. 12mo. Pp. 214.

Articles on Reform in Private Asylums. By Henry Monro, M.B., Oxon.; Fellow of the Royal College of Physicians, &c. &c. London, 1852. 8vo. Pp. 87. Pp. 23.

On the Enlargement of Articular Extremities of Bones in Chronic Rheumatic Arthritis. By W. Adams, F.R.C.S., Assistant Surgeon to the Royal Orthopædic Hospital; Demonstrator of Morbid Anatomy at St Thomas's Hospital; Fellow of the Royal Medical and Chirurgical Society; and one of the Council of the Pathological Society of London, &c, From the Third Volume of the Transactions of the Pathological Society of London. London, 1851. 8vo. Pp. 16. Lithographed Delineations.

An Essay on Unhealthy Inflammation. By M. Broke Gallway, Surgeon, Royal Regiment of Artillery. London, 1852. 8vo. Pp. 90.

The Spirometer, the Stethoscope, and Scale-Balance; their Use in discriminating Diseases of the Chest, and their value in Life-Offices; with Remarks on the Selection of Lives for Life Assurance Companies. By John Hutchinson, M.D., Assistant Physician to the Brompton Hospital for Consumption, &c. &c. London, 1852. 8vo. Pp. 79.

The Symptoms and Treatment of the Diseases of Pregnancy. By William John Anderson, F.R.C.S., District Accoucheur to St Mary's Hospital. London, 1852. Post 8vo. Pp. 119.

Further Observations on that portion of the Second Report on Quarantine, by the General Board of Health, which relates to the Yellow Fever Epidemy, on board H. M. S. Eclair, and at Boavista, in the Cape de Verde Islands. By J. O. M'William, M.D., F.R.S., R.N., Medical Inspector of Her Majesty's Customs. London, 1852. 12mo. Pp. 39.

Report by the Central Board of Health of Jamaica. Spanish Town, 1852. 8vo. Pp. 289.

Appendix to the Report of the Central Board of Health of Jamaica. Spanish Town, 1852. 8vo. Pp. 272.

The London Dispensatory: a Practical Synopsis of Materia Medica, Pharmacy, and Therapeutics; Illustrated with many useful Tables and Woodcuts of the Pharmaceutical Apparatus. By the late Antony Todd Thomson, M.D.. F.L.S., Professor of Materia Medica, Therapeutics, and Medical Jurisprudence in University College, London; Fellow of the Royal College of Physicians of London; Fellow of the Medical, the Speculative, and the Royal Physical, Societies of Edinburgh, &c. &c. Eleventh Edition. Edited by Alfred Baring Garrod, M.D., Licentiate of the Royal College of Physicians; Professor of Materia Medica and Therapeutics in University College, London; and Physician to University College Hospital, &c. &c. London, 1852. 8vo. Pp. 1280.

A Practical Treatise on Diseases of the Skin. By J. Moore Neligan, M.D., M.R.I.A.; Honorary Fellow of the Society of Physicians of Sweden; Physician to the Jervis Street Hospital; and Lecturer on Practice of Medicine in the Dublin School of Medicine. Dublin, 1852. Post 8vo. Pp. 439.

The Westminster Review, No. cxii. April 1852. London, 1852.

The British and Foreign Medico-Chirurgical Review; or, Quarterly Journal of Practical Medicine and Surgery, No. xviii. April, 1852. London.

The London Journal of Medicine, No. xl., April; No. xli., May; No. xlii., June, 1852.

The American Journal of Medical Sciences. New Series, No. 46. April, 1852. Philadelphia.

The Dublin Quarterly Journal of Medical Science. No. xxvi., February 1852; and No. xxvii., May 1852.

Weekly Tables of Mortality for the Metropolis. No. 13, Vol. xiii., week ending Saturday, 27th March 1852, to No. 25, Vol. xiii., week ending June 19, 1852.

Journal fur Kinder-Krankheiten (Journal for Diseases of Children). Herausgegeben von Dr Fr. J. Behrend und Dr A. Hildebrand. Erlangen, Band xviii., Heft 1 und 2, Jan. Feb., 3 und 4, Marz—April, 1852.

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A Treatise on the Practice of Medicine. By George B. Wood, M.D., Professor of the Theory and Practice of Medicine, in the University of Pennsylvania; President of the College of Physicians of Philadelphia; one of the Physicians of the Pennsylvania Hospital; one of the Authors of the Dispensatory of the United States of America, &c., &c. Third Edition. In Two Volumes. Philadelphia, 1852. Vol. I., Pp. 847. Vol. II., Pp. 853.

On the Anatomy and Physiology of the Male Urethra, and on the Pathology of Strictures of that Canal. By Henry Hancock, F.R.C.S., House Surgeon and Lecturer on Surgery, Charing-Cross Hospital; Surgeon to the Royal Westminster Ophthalmic Hospital, &c., &c. London, 1852. 8vo. Pp. 86.

Biography of Dr Sheridan Muspratt, F.R.S.E., M.R.I.A., F.C.S., Professor in the College of Chemistry, Liverpool. By a London Barrister-at-Law. And a third Edition of the Influence of Chemistry on the Animal, Vegetable, and Mineral Kingdom. By Dr Sheridan Muspratt. London, 1852. 8vo. Pp. 32.

Gluckwunsch zur Funfzigjahrigen Doctor Jubelfeier Des Herrn Georg Wilhelm Franz Wenderoth, Doctors der Medicin und Philosophie, ordentlich offentlichen Professors der Medecin und Botanik, Directors der Botanischen Gartens u. s. w. Als Zeichen der Verehrung und Anerkennung seiner Verdienste im Namem der Medizinischen Facultat zu Marburg, dargebracht am 31 December 1851. Durch, W. Roser, Professor der Chirurgie, Marburg, 1851. 4to. Pp. 44.

On the Diseases of the Kidney, their Pathology, Diagnosis, and Treatment; with an Introductory Chapter on the Anatomy and Physiology of the Kidney. By George Johnson, M.D., London, Fellow of the Royal College of Physicians; Assistant Physician to King's College Hospital. London, 1852. 8vo. Pp. 517.

Military Surgery; or, Experience of Field Practice in India, during the years 1848 and 1849. By J. J. Cole, M.R.C.S.E., H.E.I.C.S., late Surgeon to the Auxiliary Forces during the War in the Punjaub. London, 1852. 8vo.

Practical Remarks on Palpitation, and other Functional Diseases of the Heart. By John Calthrop Williams, M.D., Fellow of the Royal College of Physicians, Edinburgh; Visiting Physician to the General Lunatic Asylum, near Nottingham; Honorary Medical Governor for Life, and late Physician to the General Hospital, near Nottingham, &c. Second Edition. London, 1852. 8vo. Pp. 237.

Medical Jurisprudence. By Alfred Taylor, M.D., F.R.S., Hn. M.D., University, St Andrews; Licentiate of the Royal College of Physicians; Member of the Royal College of Surgeons; and Lecturer on Medical Jurisprudence and Chemistry in Guy's Hospital. Fourth Edition. London, 1852. Small 8vo. Pp. 826.

On Animal Electricity; being an Abstract of the Discoveries of Emil Du Bois Reymond, Member of the Academy of Sciences at Berlin, &c., &c. Edited by H. Bence Jones, M.D., A.M., CANTAB., F.R.S., &c.; Physician to St George's Hospital. London, 1852. 12mo. Pp. 214.

Disease in Childhood; its Common Causes, and Directions for its Practical Management. By Robert Ellis, F.L.S., Surgeon to the National Society's Training Institution for Schoolmistresses, &c. London, 1852. 12mo. Pp. 288.

The Russian Bath; published with a view to recommend its introduction into England for Hygienic as well as Curative Purposes. By Mathias Roth, M.D., Physician to the Hahnemann Hospital; Author of the Prevention and Cure of Chronic Diseases by Movements, &c. London, 1852. 8vo. Pp. 38.

Movements or Exercises according to Ling's System, for the Development and Strengthening of the Human Body in Childhood and Youth. By M. Roth, M.D., &c. Extracted from Dr Roth's Larger Book, The Prevention and Cure of Diseases by Movements, &c. London, 1852. 8vo. Pp. 41.

The Medical Report of the Female Side of the Colney Hatch Lunatic Asylum, Middlesex. By James George Davey, M.D., &c.; Physician and Superintendent of the Female Department of the Asylum. London, 1852. 8vo. Pp. 48.

The Dictionary of Domestic Medicine and Household Surgery. By Spencer Thomson, M.D., L.R.C.S.E. Part viii. and Part ix. London, 1852.—None of the Numbers except the first and seventh, and those now mentioned, have been received.

Solanumania, or the Potato Disease; Its Origin and History; Progress or *modus operandi;* its true and only Remedy, &c., &c. By G. Bartlett, London. 12mo. Pp. 26.

Practical Observations on Diseases of the Lungs and Heart. By Archibald Billing, M.D., A.M., F.R.S., Fellow of the Royal College of Physicians, Member of the Senate of the University of London, &c. London, 1852. 8vo. Pp. 138.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine and Surgery. No. xix. July 1852.

The London Journal of Medicine, No. xliii., July; No. xliv., August; No. xlv., September 1852.

The American Journal of Medical Sciences. New Series. No. xlvii., July 1852. Philadelphia.

The Dublin Quarterly Journal of Medical Science. No. xxviii., August 1852.

Weekly Tables of Mortality for the Metropolis. No. 26, Vol. xiii., week ending June 26th 1852, to No 38, Vol. xiii., week ending Saturday, 18th September 1852.

Appendix (C) to the Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849. Abstract of Report by James Wynne, M.D., on Epidemic Cholera as it prevailed in the United States, in 1848 and 1849. Presented to both Houses of Parliament by command of Her Majesty. London, 1852. 8vo. Pp. 93.

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PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*On the Impregnation of the Ovum in the Amphibia. Section First.* By George Newport, F.R.S., &c. (From the Philosophical Transactions of the Royal Society for 1851. Part I., p. 203.) Received 20th June.—Read 20th June 1850. (*Concluded from Volume seventy-seven, p.* 385.)

4. THE AGENCY OF THE SPERMATOZOA IN IMPREGNATION.

It is evident from the last-mentioned experiments, that however great may be the influence of temperature in accelerating or retarding impregnation and development, and however much the operation of this influence may be interfered with by want of proper aëration of the ovum, the impregnating force does not equally pertain to all parts of the seminal fluid, but is to be found in some only of its constituents. Experiments made before those now detailed,—and to which I had been led by a conviction that the opinion formerly entertained, that impregnation is effected through means of the fluid portion only of the semen, was not in accordance with facts I had very long been acquainted with in the Articulata,—convinced me that the spermatozoa themselves, and not the other constituents of the semen, are the efficient agents of impregnation. Leewenhoek, and, as I have since found, Prevost and Dumas, not only believed this to be the fact, but also held the opinion that the spermatozoa penetrate bodily into the ovum; and this view has been more recently insisted on by Dr Martin Barry, with the additional belief, that a perforation or fissure exists in the envelopes of the ovum, through which the spermatozoon enters. On careful examination of the envelopes of the ovum of the frog, I have not been able to detect any fissure or orifice. The question of the agency of the spermatozoa, nevertheless, appears to be capable of solution, however difficult it may be to ascertain the mode and particular nature of such agency. The separation, as far as possible, of the spermatozoa, by filtration from the fluid in which they move, and testing the ova with these and the fluid separately, afford good proof of the agency of these bodies; while immersion of the ova in coloured fluids, at the moment of their passage from the body of the frog, seems equally fitted to ascertain the believed existence of a fissure or perforation through the envelopes during their expansion. The experiment of filtration was originally performed by PREVOST and DUMAS with well-marked results, and it has since been repeated by the first of these observers* by a different mode,—endosmose through the operation of galvanic currents. The mode pursued by myself was that originally adopted by these observers:—careful mechanical filtration, by simply passing the fluid portion of diluted semen through folds of filtering-paper. The paper I have employed, and which alone was fitted for the purpose, was the best Swedish filtering-paper employed by chemists in their most delicate analyses. A large proportion of the spermatozoa were always retained, even on a single filter, although a few usually passed through; but this, as the results shew, was not in reality a disadvantage, when a few only were present in the filtered portion. When three or four folds of filtering-paper were employed, the whole of the spermatozoa were removed.

Filtration of Seminal Fluid.—Fluid obtained from a male frog, immediately after removal from the female, was mixed with about twice its quantity of water and placed on the filter. Portions of this fluid as they passed through were repeatedly examined with the microscope. Some of these filtered specimens contained a very few spermatozoa, usually not more than three or four in the drop examined, but sufficient occasionally, as the results proved, to effect impregnation.

These experiments were in number eight. Four were performed with filtered seminal fluid, and four with fluid not previously subjected to filtration. In both sets, to the seminal fluid, water, generally to the amount of one ounce, was added. The number of ova added and immersed in the united fluid, with the results, are shewn by the following table:—

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* Journal de l'Institut, 1840, No. 362, p. 908.

No.	Ova.	Results.
1	46	No segmentation. No embryo produced.
2	90	None segmented.
3	60	None impregnated.
4	105	Two partially impregnated, but no embryos produced.
5	76	Several segmented. 15 embryos produced.
6	110	Two segmented, two embryos.
7	150	Segmentation in two or three; in five hours in all; embryos produced.
8	200	A few imperfectly segmented; no embryo produced.

From these first experiments with filtered fluid, it seemed that the portion of semen which passes through the filter has not the power of impregnating, unless there are spermatozoa present in it; while similar quantities of diluted semen that has not been filtered, are efficient and impregnate, as in Nos. 4, 5, 6, and 7. Further, it is shewn, from No. 8, if we may judge from one experiment, that ova which have remained in the body of a frog twenty hours after actual death and cessation of the organic functions, and in a temperature of 55° Fahr., may be affected by the stimulus of the impregnating fluid, but not sufficiently so perhaps as to result in fruitful impregnation.

During the time these ova were under observation, in March 1849, an opportunity occurred of observing the effect of reduced temperature on the rate of development of the embryo when its formation has been somewhat advanced. On the seventh, eighth, and ninth days after impregnation of the ovum, and when the temperature has already been considerably reduced, the season became severe, and in order to test the effects of cold, the eggs were removed to the open air and exposed to a keen wind. The temperature of the atmosphere was then 38° Fahr. During the night of the tenth day, the water in which the ova were contained was frozen to a mass of ice. Yet many of these ova, as above shewn, produced embryos. SPALLANZANI had already remarked, that the eggs of the frog may be inclosed in ice, and yet afterwards produce embryos, if the envelope does not become frozen.*

The experiments made to ascertain whether cleavage of the yelk may be taken as a test of impregnation (p. 190), seemed also to shew that, *within certain limits*, a large or small quantity of seminal fluid has some influence on the more or less early occurrence of this phenomenon. It occurred to me, therefore, that in making the experiments now given, two questions might be examined: one, as to whether the extremely minute quantities of seminal fluid disseminated in water, as mentioned by SPALLANZANI, are as efficient to produce the embryo at the low temperature of the season at which the frog spawns in this country, as in the warmer region of Italy; and the other, whether the presumed efficiency of such minute quantities depended on the presence of the spermatozoa; and it seemed possible to put these questions to the test in one set of experiments. But, in attempting to do so, it was a matter of importance, first, to obtain some approximative knowledge of the actual quantity of spermatic fluid employed by SPALLANZANI, in his more remarkable experiments, to enable me to compare the results of my observations with those of his. SPALLANZANI states, that by his mode of obtaining the seminal fluid, by vivisection, he could only procure from "two to three grains"* from each individual. Three *grains* weight of fluid are equal, by measure, to nearly three *minims* of our medicinal standard. But it may be presumed that in so delicate an operation as that of removing the fluid from the vesiculæ seminales, from which SPALLANZANI states he usually obtained it, he could rarely be very precise in his determination of the quantity. I assume, therefore, for the sake of comparison, that the quantity he speaks of as "a *grain*," was about equal to a *minim* of our medicinal measure. Three grains of fluid (? minims), he says, were mixed with a *pint and a half* of water, and one *drop* of this mixture (the seminal fluid in which was equal to $\frac{1}{3840}$th part of the whole at 16 oz. per pint) applied directly to an ovum on the point of a needle, was "frequently" sufficient to render it fruitful.† The *drop* spoken of in this experiment was a much less quantity than the grain or minim; indeed, SPALLANZANI states that it did not exceed the "fiftieth of a line" (? fifth) in diameter. The quantity of fluid obtained by myself from a frog was usually about *six minims*, and this I mixed with twice its quantity of water, thus making eighteen minims. One *drop* of this mixed fluid measured, as I found, one-third of a minim, and consequently contained one-third part of seminal fluid, or one-ninth of a minim of the seminal fluid. Yet the result of the employment of one drop of this mixed fluid added to one ounce of water, in which the proportion of seminal fluid was then made to be $\frac{1}{1310}$th part of the whole, did not lead to the same result as the experiment by SPALLANZANI. The difference arose, perhaps, from the operation of two or more causes :—first, the much lower temperature of the atmosphere at the time of making my experiment, than at that of SPALLANZANI'S ; next, the diminished efficiency of the seminal fluid, owing to the length of time (two hours) it had been removed from the body. Both these circumstances, but especially the first, as already shewn, operate unfavourably in experiments on impregnation, more eggs being fertilised at a high temperature, when the changes go on rapidly, and especially when the seminal fluid has been most recently obtained, than under the opposite conditions. The general results of my experiments, however, may be regarded as quite confirmatory of SPALLANZANI'S more remarkable one, as they prove, like that, that only an exceedingly small proportion of seminal fluid is necessary to fertilise the ovum.

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* Dissertations, &c., vol. ii. p. 49.

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* *Loc. cit.*, vol. ii., p. 189.

† *Loc. cit.*, vol. ii., p. 192. There is some confusion of statement in the translation of SPALLANZANI's work, now referred to, respecting the quantities mentioned by the author, as "pint" is the word used in some passages, and "pound" in others (p. 191), apparently synonymously, while the latter is further spoken of as "twelve ounces."—G. N.

But however satisfactory these experiments were with regard to that fact, they still left the question of the immediate agency of the spermatozoa in impregnation in doubt. I therefore repeated them with greater precision, and with that object only in view, and took especial care to obtain as perfect a filtration and separation of the spermatozoa from the fluid as possible. The results were far more interesting and instructive than I had anticipated, as I found by repeated examination by the microscope that the filtered fluid was almost completely deprived of spermatozoa, one or two only being occasionally detected in it, with a very few nuclei and spermatozoal cells. One circumstance that tended to increase the value of this set of experiments, and to prove the influence of the spermatozoa, was, that the ova employed were not fully matured, and hence I had less expectation of a favourable result. The quantity of seminal fluid obtained was larger than usual, and this was mixed with twice its quantity of water. This mixed fluid was divided, as before, into two portions, one of which was filtered, and the other not. The experiments were commenced in the early part of the day, and the temperature of the atmosphere of the room, and that of the water employed, was nearly the same, 51° Fahr., and the whole of the experiments were placed as nearly as possible under similar circumstances.

Set L. The results obtained from these experiments, which were performed on the 18th of March 1849, when the temperature of the atmosphere and of the water was at 51° Fahr., are given in the following table.

	Minims of Mixed Fluid.	Water.	Ova.	Results.
No. 1	10	2 ounces	150	Segmentation commenced in five hours forty minutes in a few ova; in others at a later period. A few only produced embryos.
	Filtered fluid.			
No. 2	10	2 ounces	150	At the end of one hour, no spermatozoon observed. No cleavage at the end of five or six hours. No embryo produced.

As the ova in these two experiments were the first that passed from the body of the frog, it was reasonable to regard them as being the most matured. Segmentation ought therefore, if it occurred at all, to have taken place at an earlier period in these than in others afterwards obtained from the same female.

No. 3. The *filter paper* employed in separating the fluid used in the last experiment, No. 2, and which retained the separated spermatozoa in a minute quantity of fluid that had not passed through, was placed in two ounces of water, and *one hundred and thirty ova* from the same female were shed upon it. When some of these ova were examined at the end of an hour and a half, spermatozoa in vast abundance were found adhering to every part of their surface, but the whole were then motionless, and apparently dead and partially coiled on themselves. In some of these the body appeared to be slightly enlarged. Segmentation commenced in very many of these ova *in five hours and ten minutes*, and was almost universal in them a few minutes later. *Nearly the whole of these ova produced embryos*, there being only nine out of the one hundred and thirty that were abortive. I have sometimes found a much larger proportional number of unproductive ova in some broods in the natural haunts of the frog.

There was one exceedingly interesting fact in this experiment. This was that the smaller and apparently less matured ova were as fully impregnated as the larger and more perfect. The whole set of observations was the more interesting from this circumstance. These ova were smaller than usual, and had not the white surface so complete, but were very like the ova in which I have described the changes in the light-coloured surface (vol. lxxvii., p. 368). When first placed in water with the filter paper and spermatozoa, the surface of the yelks became more contracted and irregular, within the vitelline membrane, than in other ova I have employed. Hence I had much doubt, at first, whether any satisfactory evidence would be obtained from this set. But the contrary has been the case, as it is evident that some ova may be impregnated at a little earlier period than usual; and that when a great abundance of spermatozoa is supplied, ova less matured than others may be equally well impregnated. The greater efficiency of a larger as compared with a smaller number of spermatozoa, with reference to the earlier or later segmentation of the yelk, has already been shewn; and the difference is very marked in the first and third of this set of experiments, both with reference to the occurrence of segmentation and to the relative fecundity of the ova. On the other hand, the experiment No. 2 proved that the *liquor seminis is not* the fecundating portion of the seminal fluid.

Circumstances having prevented me from making known the result of these experiments at the time they were obtained, I determined, during the past spring, to repeat and vary them, to obtain, if possible, still more conclusive proofs that the spermatozoa alone effect impregnation. Accordingly, in March and April of the present year (1850) I repeated them, with the following precautions:—first, that the frogs in each case had been some days paired, and at the time of the experiment were nearly ready to spawn; next, that the seminal fluid used was obtained from the male paired with the female from which the eggs were taken; further, that the condition of the specimen of fluid used was correctly ascertained; and lastly, that the ova were placed in flat dishes, under precisely similar circumstances, with similar quantities of water, repeatedly changed. Two sets of experiments were made at the same time.

Set M. April 4, 1850. Atmosphere 60° Fahr.

The seminal fluid employed, mixed with an equal quantity of water, was placed on a *single*, and caught on a double filter paper, and the clear fluid that passed was then examined with the microscope. The fluid that had traversed the three filters was almost completely deprived of spermatozoa; as, after many very careful examinations, both of drops as they passed the last filter, and of portions from the capsule it was caught in, there was only one instance of the presence of spermatozoa. In this I saw *two*, but perfectly motionless. The fluid that passed through the *first* or top filter, I had reason to suspect was contaminated by a small quantity having travelled over the sides of the filter. I knew also, that a few spermatozoa always penetrate through a single filter, or rather perhaps are carried through by the fluid. Accordingly, I detected several spermatozoa in this portion. The fluid that remained in the interior of this filter abounded with very active ones. It was not employed until fifty-three minutes after it had been procured from the frog.

No. 1. *Seventy ova* were passed on a dry surface, and a portion of the fluid which had traversed three filters was poured carefully over them, and water was then added.

In *four hours and twenty-five minutes* I found *one ovum* partially impregnated, and the yelks of others were somewhat shrunk, and a little irregular, an appearance frequently seen in ova that have not been impregnated. Not a single ovum, however, was fruitful.

No. 2. *One hundred and twenty-seven ova* were shed into water on the second of the three filters, which still retained a portion of the fluid from the first, and in which I had found a few spermatozoa. At *four hours and fifty minutes* several of the ova had begun to be divided, and out of this set of eggs *sixteen embryos* were produced.

No. 3. *One hundred and sixty-three ova* were passed into water on the topmost of the three filters, and *four hours and fifty minutes afterwards* a very large proportion of them had become segmented, as was expected, although from the circumstance of the seminal fluid having been more than one hour mixed with water before it was employed, the number was not so great as usual. But it was not entirely due to this cause. Many of the ova had been injured before impregnation, and these became irregular and pear-shaped. At the end of twelve days *forty-nine* well-developed embryos had been formed.

Set N. April 4, 1850.

Four filters were now used instead of three, the seminal fluid being obtained and mixed as before. The fluid which had passed through the whole of these filters, contained not a trace of spermatozoa or of the nuclei of cells. That which had passed through two only still gave an occasional perfectly motionless spermatozoon, but not a single one in motion; the fluid from the uppermost or first filter, as in *Set* M., still swarmed with myriads of active spermatozoa. It was thus proved that the filtration was complete.

No. 1. *One hundred and thirty-one ova* were passed on a dry surface, and a portion of the fluid which had traversed the four filters was poured over them, and water was then added. But not a single ovum gave any sign of having been impregnated, not one became segmented, nor was a single embryo produced. The ova became slightly irregular, shrunken, and depressed at parts of the yelk, a condition which, as I had noticed this in other ova, I was inclined to attribute to some normal change in the yelk itself, perhaps of an imbibing or endosmic character.

No. 2. *One hundred and ninety-seven ova* were shed into water mixed with the remaining portion of fluid which had passed through the four filters, but not a single egg gave even a trace of segmentation. Many of the yelks had the same peculiarly irregular outline as in No. 1. These two experiments I regarded as a satisfactory proof that it is not the fluid portion of the semen which impregnates.

No. 3. *Two hundred and four ova* were passed into water upon the third filter, already immersed in it, and the fluid on which shewed an occasional spermatozoon. I was unable to detect any impregnated ova in this experiment, but, as the result shewed, a few had been affected, as *four embryos* were produced.

No. 4. *Three hundred and seventy-one ova* were passed into water upon the first or topmost of the four filters, and which had already been placed in the water. At *four hours and thirty minutes*, almost every yelk had become segmented. The change had occurred some length of time before this, as the second or crucial segmentation was commenced. This experiment seemed to be a most direct and conclusive proof of the agency of the spermatozoa. At the end of four days almost the whole of the eggs were producing embryos, many of which were advanced to the *fourth period of development. One hundred and twenty-seven* became fully formed and vigorous, besides nearly as many more which did not complete their changes, from an accidental cause.

Before these concluding experiments were made, I had already, in March last, repeated the preceding; but, as the filtration was less perfect, have thought it unnecessary to give them in detail; they agreed however in the results. The whole have confirmed in the fullest manner the experiments first made, and have proved, as I trust, satisfactorily that the spermatozoa alone are those parts of the semen which effect the impregnation of the ovum. Having repeated the filtration in five separate sets of experiments, on different occasions, and with exactly the same general results, I can no longer entertain any doubt of the direct agency of the spermatozoa. The conclusion, I think, is rendered certain by facts now shewn, which escaped the notice of SPALLANZANI, and of PREVOST and DUMAS. Segmentation of the yelk takes place earlier when impregnation is effected by a large, than when occasioned by a very small number of spermatozoa, the temperature of the surrounding medium, and all other circumstances, being alike in the two cases, as in the experiments *Set* A. as compared with *Set* B. (vol. lxxvii., p. 374). This fact is supported by another, equally significant. When only a very small number of spermatozoa exist in the fluid, then the remarkable result of partial impregnation often takes place, and the ova are unproductive. On the other hand, when spermatozoa are supplied in full abundance to the ova, not only does segmentation of the yelks take place more rapidly, but also more extensively, and almost every ovum produces an embryo.

With regard to the *liquor seminis*, it seems equally decisive that this portion of the seminal fluid *does not* effect impregnation. WAGNER and LEUCKARDT* have justly remarked, that it is almost impossible that the *liquor seminis* can have any action on ova which are expelled into water before the semen is ejected by the male, as in the case of frogs and fishes; and it is worthy of note. that in the experiments now detailed not a single ovum was either *completely* or *partially* impregnated when immersed in water mixed only with the liquor seminis, obtained through filtration; nor even when the ova were carefully bathed, as they passed from the body of the Frog, and before they had been brought into contact with water, with the filtered fluid from which the spermatozoa had been completely separated, as in *Set* N, No. 1, or even when the fluid still contained a very few dead and perfectly motionless spermatozoa. Yet this fluid can hardly be regarded as entirely without use, although it now appears to be of very secondary consequence. When the ova were placed in water with which only the *liquor seminis* had been mixed, the yelks became contracted exactly as is often the case in the unimpregnated ovum. Whatever may be the nature of this fluid, it does not appear to be essential to the conveyance of the structural peculiarities of the male parent to the offspring. These appear to be communicated by the spermatozoa alone, as not only did the ova that were impregnated by spermatozoa from the filter paper, as in *Set* N, No. 4, become segmented quickly, but the embryos produced from them came forth with all the usual characters of tadpoles, and have passed, or are now passing (June 20) through their stages of growth as perfectly and as quickly as others which have been produced in the natural haunts of the species through the mutual concurrence of both sexes. Thus the *liquor seminis* does not even hasten the course of development of the young. Neither does it accelerate that of fecun-

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* Cyclopædia of Anatomy and Physiology, vol. iv. "Semen," part xxxiv. January 1849, p. 507.

dation, either through direct imbibition or from becoming a solvent to the bodies of the spermatozoa; as we have seen that segmentation of the yelk takes place most quickly in proportion to the number of spermatozoa, within certain limits, in contact with the ovum. And such also is the case in a state of nature.

These facts appear to give that *direct negative* and refutation to the hypothesis of the immediate agency of the *liquor seminis* in impregnation which WAGNER and LEUCKARDT* remark it has not hitherto met with; and they lead to the supposition that one of the chief uses of the fluid is merely that of a vehicle through which the spermatozoa are more readily brought into contact with the ova. Possibly it may bear that relation to the spermatozoa in the viviparous vertebrata, in which it chiefly occurs, which the fluid medium into which the ova of Amphibia and Fishes are expelled, bears to the spermatozoa in those classes. This view may derive some support from the fact that the *liquor seminis* has recently been shewn by chemical analysis to consist chiefly of a thin solution of mucus, with small quantities of chloride of sodium and phosphates and sulphates of the alkalies.†

5. NATURE OF THE AGENCY OF THE SPERMATOZOA.

Having obtained, by direct experiment, full evidence that impregnation is effected through means of the spermatozoa, we have now to inquire as to the manner in which it is induced by them, and as to the nature of the agency they exert. Spermatozoa have been found adhering to the surface of the impregnated ovum by BARRY, BISCHOFF, POUCHET, and others, in the Mammalia; as they were long ago seen by PREVOST and DUMAS on the ovum of the Amphibia; and since by SIEBOLD, KÖLLIKER, myself and others in the Invertebrata. They have been constantly present in those experiments on artificial impregnation which I have now detailed, in which the yelks became segmented after the egg had been in contact with seminal fluid, or with the filter paper used to remove them from the *liquor sanguinis;* but they have not been detected on ova which did not undergo the cleavage of the yelk, or which had been immersed only in fluid separated during filtration. Whenever present in fluid in which ova have become impregnated, the spermatozoa have always been found in motion, until after they have become attached to the surface of the ovum, when their motion has soon ceased. In the *Lissotritons*, in the few experiments I have made on this division of the Amphibia, I have seen the motion continue for a long time after they have been in contact with ova. This leads to the supposition that a vibratile condition or power of motion is in some way essential to their power to effect impregnation; notwithstanding that, as WAGNER and LEUCKARDT have remarked*, no movements have as yet been perceived in the spermatozoa of the *Isopoda* and *Amphipoda*. If this vibratile condition be essential to their function, then the length of time which it is continued may be of importance.

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* *Loc cit.*, p. 507.

† *Ib.*

Duration of Motive Power in Spermatozoa.—SPALLANZANI found that water mixed with but a small quantity of seminal fluid of the Frog retained the property of impregnating ova longer than pure semen.† When inclosed in a glass tube, the semen of the Toad was not impaired at the end of six hours, but was useless at the end of nine;‡ while a small quantity from the Frog, mixed with water and preserved at a temperature of about 40° Fahr.,§ was still efficient at the end of thirty-five hours. Results obtained by myself have fallen short of this extended period, even when the influence of temperature has been attended to. The difference of result may to some extent be accounted for in the condition of the fluid, and in the way in which it has been obtained. SPALLANZANI says he obtained it both from the *vesiculæ seminales* and the testes after opening the body. It is probable therefore that a large part of what he procured had not arrived at maturity, and instead of consisting almost entirely of active spermatozoa, as when it is obtained by compression of the body, the mode adopted in my experiments, it included a large proportion of developmental cells from which the spermatozoa escaped at longer or shorter periods after the fluid had been mixed with water, or had been retained for some time out of the body. I have very rarely found the seminal fluid of *Rana temporaria* obtained in the way stated, and at a temperature of about 50° Fahr., retain any impregnating influence for more than four or five hours. Thus, after mixing it with an equal proportion of water, very many of the spermatozoa have soon become motionless, and in less than two hours a moderate proportion only have continued active. At three hours there have been still fewer moving; while at four hours the great majority of them have exhibited most unequivocal signs of lost vitality, being either extended at length or coiled on themselves, as they usually appear when motionless, and adherent to the surface of ova. If any, at this length of time, have been still moving in the fluid, they have been few in number, and their motions exceedingly feeble. Occasionally I have detected others, at this lapse of time, in the act of escaping from the cells, and these have always been the most energetic in their movements immediately after their liberation. Further, I have noticed that in those specimens of fluid which have contained most developmental cells, the spermatozoa have been longest in a state of activity.

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* *Loc. cit.*, p. 503. † Dissertations, vol. ii. p. 193. ‡ *Id.* p. 168.

§ It may be well here to mention, that for the purpose of more easily comparing the observations recorded by SPALLANZANI, and PREVOST and DUMAS, with the results obtained by myself, I have, throughout this paper, reduced the data given by them to the scale of Fahrenheit, employed by myself; the scale used by SPALLANZANI being that of REAUMUR, and by PREVOST and DUMAS the Centigrade.

The following have been the results of observations on spermatozoa attached to the surface of ova, or contained in the water in which ova were immersed. At *three-quarters of an hour* after mixing *recently obtained* seminal fluid with the water and ova, vibratile spermatozoa have continued to be exceedingly abundant and in a state of great activity. *At one hour and a quarter* there were still an abundance in motion, but many were now perfectly motionless, and apparently dead. At *one hour and a half* I was not able to detect any movement in even a single spermatozoon out of a vast abundance which adhered to the surface of the gelatinous coverings of the ova, although I sought for this very carefully. Neither could I detect even the slightest indication of the spermatozoa having penetrated into these coverings, either near the surface or in the vicinity of the thicker envelope, which I regard as the *chorion*, and which immediately covers the *vitelline membrane*. After a lapse of some time all the appearance of spermatozoa on the exterior of the envelopes ceased. The longest period, after contact with spermatic fluid in water, at which I have hitherto been able to recognise these bodies on the surface of the frog's egg has been *six hours and one or two minutes*, and about half an hour after segmentation of the yelk had commenced. This was on ova impregnated artificially, on the 14th of March, at a temperature of the atmosphere of the room of 54°·5 Fahr. and 53°·5 of the water employed. A few motionless spermatozoa were then still found on the surface, but most of them appeared to be becoming disintegrated. The surface of the egg-envelope was then covered at places with numerous small granules, possibly the remains of spermatozoa which had disappeared.

A somewhat similar result has ensued when spermatozoa have been *two hours* mixed with water before ova were passed into it. At *five minutes* after immersion of these ova, I have found large quantities of spermatozoa already adhering to the surface of their then expanding envelopes. But many of them have already been coiled on themselves, and were perfectly motionless. The water still contained very many disseminated through every part of it, but most of them, with few excèptions, have appeared to be rigid, and to have become enlarged in diameter, but not increased in length. This change has appeared to be due to the hygroscopic nature of these bodies, as formerly pointed out by SIEBOLD.* Possibly this nature may have some reference to impregnation. Repeated observations lead me to believe that, in whatever way the spermatozoa are concerned in impregnation, they do not penetrate bodily into the ovum, but merely adhere to the surface.

PREVOST and DUMAS concluded† from their investigations, that the spermatozoa of the Triton and Frog do penetrate into the envelope of the egg; and they state that they had fecundated ova taken from the ducts of the Triton, and after the lapse of three hours, having first carefully washed them, to remove all that were merely adhering to the surface, have made sections of the envelopes of the egg, and, with the aid of the microscope, have found living spermatozoa still struggling within. Their words are,—" Une grande quantité d'animalcules encore mouvans, et qui semblaient se débattre dans cette espèce de gelée où ils se trouvaient emprisonnés. On en voyait partout même au contact des membranes de l'œuf." They further state, that they had repeated this experiment on the ova of the Frog, and found the envelope penetrated in like manner with spermatozoa, still moving, but not changing place. I regret much that my investigations do not enable me to confirm these observations, which seem to me to be due to the circumstance of these physiologists having regarded the objects on the surface as being in the interior. I have many times sought for spermatozoa within the substance of the egg-envelope of the Frog, at different periods between that of first contact with impregnating fluid and the time when cleavage of the yelk has commenced, and have constantly found them on the surface, but have never, even in a single instance, observed any appearance of them in the substance of the envelope, nor anything which induced me to suspect that they penetrate bodily into it. They have been present on the surface, and adherent to it, even from within a few seconds after contact, to so late as the sixth hour, but have usually been motionless; and most of them have had the caudal portion folded back on the body. I was led to make these observations on the egg of the Frog, before I was aware of the views entertained by MM. PREVOST and DUMAS,—from the circumstance of Dr MARTIN BARRY having mentioned* that an orifice or fissure exists in the thick investing membrane of the ovum of the Rabbit, through which, at the time of impregnation, he believed the spermatozoon to enter. All the observations I have been able to make on the ovum of the Frog, both microscopically and experimentally, are opposed to the belief in the existence of any perforation either before or at the time of impregnation. With regard to the ovum of the Tritons, I have recently made the following observations, since becoming acquainted with the views of the authors named.

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* MULLER's Archiv., 1836.

† *Loc cit.*, vol. ii. p. 133.

A female *Lissotriton punctatus*, obtained on the 17th of May, produced several ova on the morning of the 19th. Cleavage of the yelk (which, I may remark, was entirely overlooked by RUSCONI† in his account of the Newts) commenced in two of these ova at the expiration of eight hours, the temperature during the period having ranged from 56° Fahr. to 62° Fahr. On examining these ova very carefully about an hour before the cleavage commenced, there were what I regarded as portions of the bodies of spermatozoa on the surface, but certainly no traces of any in the interior. While engaged in this examination the same female produced another egg, which she inclosed as usual in a folded leaf. On this specimen, examined at the end of an hour, I could not detect any spermatozoa on the surface, which led me to imagine that it had not been impregnated, a supposition which ultimately proved to be correct, as no segmentation of the yelk took place in it. Some time after this, the temperature of the atmosphere being 62° Fahr., I saw the same newt inclosing another egg in a leaf. This I immediately removed for examination, and thinking that this, like the previously deposited egg, had not been impregnated, no traces of spermatozoa being found on its surface, I placed it for about a minute in a small capsule filled with water, into which a quantity of fluid had just been expressed from a male that had been kept separate from the female. The fluid on examination was found to be composed almost entirely of very active spermatozoa. The egg was examined three minutes after immersion, and scarcely five minutes after it had been laid, and multitudes of spermatozoa were then seen adhering to its surface. Most of them were still vibrating rapidly, while others were motionless. But although

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* Philosophical Transactions, 1840, p. 535.

† Amours des Salmandres Aquatiques, 4to. Milan, 1821.

I was able to distinguish every part of these bodies, I could not detect any in the act of penetrating, or which had already penetrated into the substance of the envelope, and most certainly not one was embedded in the interior. Neither were there any in contact with the yelk-membrane, or in the yelk-chamber of the envelope.

The egg of the Newt is peculiarly fitted for an examination of this kind, from the fact of the existence of this yelk-chamber, or space in the interior of the envelope. This is formed by the gelatinous covering which the egg gains in the oviduct imbibing fluid by endosmose and becoming expanded immediately it comes into contact with water, when the inner layer separates from the vitellary membrane, with which it has been in contact within the duct; and as the outer layers more and more expand, the yelk, covered only by the vitellary membrane, is left free in a large cavity in the interior, surrounded by a thin fluid. The spermatozoa of the Newts, as is well known, are of large size and are easily recognised; so that in the event of their having penetrated the egg-covering before it leaves the duct, or at the moment of its expansion, or after the chamber has been formed in it, they can hardly escape observation.

I preserved this egg confined to one spot in a minute glass capsule, and in exactly the same position beneath the microscope, for forty-eight hours. During the first three quarters of an hour many of the spermatozoa on its surface exhibited as vivid motions as at first, but still adhered to the same parts, and had not, so far as I could perceive, changed their posture or their place in the least, or had penetrated in the slightest degree into the envelope. At the end of twelve hours I found that the yelk had undergone the usual process of cleavage, which, at that time, had already been advanced to the stage of coarse granulation of the surface, a fact which proved most distinctly that this egg had been impregnated. Spermatozoa were still distinctly recognised over the whole surface of the envelope, but their motions had now ceased. On the following morning, May 20th, the changes were found to have proceeded uninterruptedly, as the yelk was then finely granulated over its whole surface. At a little later period, the *end of twenty-four hours*, a few spermatozoa were still adhering to the surface of the envelope, but the whole were perfectly motionless, and many had evidently disappeared. Still, not one could be detected in the substance or in the chamber of the envelope. At *twenty-eight hours* the granulation of the yelk was nearly completed, and its surface was becoming smooth, and there were still a very few motionless spermatozoa on the exterior of the envelope. At the end of *forty-eight hours*, May 21st, I was still able to recognise the bodies of several spermatozoa which had not yet disappeared, but which had become very indistinct, as if in a state of diffluence. At the end of *sixty hours* I could no longer detect any trace of them. The egg that was the subject of these observations proceeded regularly in its changes, and ultimately produced the embryo; all the stages of which I have traced and delineated for future communication to the Royal Society.

Subsequently to these observations I saw ova passed by another female, *Lissotriton palmipes;* and on submitting these to the same close examination as the above, within five minutes after their production, I again found what I regarded as the remains of spermatozoa on the surface of the egg-covering, but not a trace of any in the interior or in the vicinity of the yelk.

Endosmosis of the envelopes during impregnation.—But, although the facts now mentioned are so opposed to the view, that the spermatozoa penetrate bodily into the ovum, it is due to the distinguished observers to whom I have referred, to enter somewhat more fully into the questions which their observations involve; and while I am free to admit the possibility of mistake or oversight on my part, to mention the details of some experiments made expressly with the view to ascertain, whether the envelopes of the ovum of the Frog are permeable in any part to solid particles of matter; or whether there exists in them any orifice by which such particles can enter. It is well known that during the transit of the ovum through the oviduct, the vitellary membrane becomes invested with a thick gelatinous covering, the first thick layer of which may be regarded as the rudimentary *chorion*, and perhaps may be analogous in its function, in some respects, as it seems to be in its place and mode of origin, to the albuminous investment of the ovum of the Rabbit. I cannot, with RUSCONI,* regard this envelope of the Frog's egg as being merely a mechanical protection during the process of development. It is formed of cells with distinct nuclei, and from what I shall presently mention, seems to be essential to the ovum at the commencement of the changes, and to be intimately connected with the impregnation. RUSCONI deprived the ova of the green aquatic frog, (?) *Rana esculenta*, of their gelatinous envelopes at a period subsequent to impregnation, and found that they passed through their changes as well as when covered by them; and he thence concluded that the envelope is of no use further than to protect the egg from the injury it might receive through mechanical disturbance, "des petits chocs qui pourroient nuire à son developpement."* Certainly it affords this protection to the germ; but to conclude that this is its sole office appears to be somewhat premature. I have found that it is almost impossible to remove this envelope from the Frog's egg at the moment of deposition, or even during the first few minutes after submersion, and before it has become expanded by imbibition of fluid; although it may be removed without much difficulty from the egg of the Newt, the yelk of which, in the vitelline membrane, lies free and unattached in its interior. But some time after the expansion has taken place, I have myself found that the frog's eggs may be deprived of a large portion of this covering, and yet produce an embryo equally well as if it had remained protected. On the other hand, one most important function of this investment seems to be indicated in the following facts. SPALLANZANI found that ova of the Frog deprived of their envelopes before contact with the male influence, were not impregnated; and further, that ova taken directly from the ovaria, are not susceptible of impregnation.†

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* *Loc. cit.*, p. 8.

A remarkable fact which I noticed, at a time when I was not fully aware of its interest and importance, enables me to confirm this observation. I captured a pair of frogs, the female of which, a short time after they were in my possession, had a large hernia formed by a protrusion of part of the great oviduct through an accidental wound in the posterior part of the right side of the body, and in consequence of which she was unable to deposit her ova. This wound had been received before the union of the sexes, but the hernia was formed afterwards, during the passing of the ova from the ovaria. The result of this was, that when some of the ova had passed into that part of the duct which protruded through the wound, the sac formed by it was constricted, and became so enlarged by the expansion of the egg-envelopes, that the remainder of the ova were prevented from entering it. On opening the abdomen of the frog after death, I found that a very large proportion of the ova which had left the ovarium on that side of the body, were lying in the cavity of the peritonæum, among the viscera, being entirely prevented from entering the duct, which was filled throughout its whole extent, to its very orifice, with eggs which had already entered, and were prevented from passing on. On the left side of the body the ova had also quitted the ovarium, but the whole on that side had passed into the oviduct in the usual way. The eggs found in the cavity of the body consisted only of yelk-masses in their vitelline membranes. I immediately placed some of these eggs in water, with seminal fluid obtained from the male with which this female had been paired; but not a single egg became impregnated, or gave afterwards any sign of formation of the embryo. I had some hesitation in regarding this experiment as quite conclusive,—that impregnation cannot take place before the egg has gained its gelatinous envelope, and consequently while it is still within, or has but just escaped from the ovary,—from the possibility that these eggs might have been for some time in the cavity of the body, and that some change might have been induced in them through long detention. In so far, however, as that this was in accordance with SPALLANZANI's experiment, it seemed to point to the nature and importance of the covering which the egg gains in the oviduct.

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* *Loc. cit.*, p. 9.

† Dissertations, &c. ii., 152-3.

Since my attention has been more particularly directed to this point of investigation, I have repeated the experiment on the ova of the Newt, *Lissotriton palmipes*, with precisely similar results. I opened the body of a female with great care (after dividing the spinal cord through the *medulla oblongata*), for the purpose of obtaining ova from the oviducts, for artificial impregnation, and immediately saw that a number of ova were free in the cavity of the abdomen, and were in the course of being transferred to the entrance of the tubes, as stated in the first part of this paper. These ova, like those which had recently escaped from the ovarium in the Frog, were without any other covering than their vitelline membranes; most certainly I was unable to detect any other, and they were so delicate that it was with difficulty they were removed into water to which fluid from the male had been added. But although uninjured in the removal, and in every way carefully treated, not one gave any sign of cleavage of the yelk, which, as I have before stated, I have constantly found take place in the impregnated ova of newts as well as of frogs, although the fact of its occurrence was overlooked by RUSCONI; not one egg afterwards produced the embryo. Thus then it seems fair to conclude, that the egg in the *Amphibia* is not fitted for impregnation until after it has entered the oviduct and acquired its gelatinous covering.

I have already shewn that there is a remarkable coincidence between the rate of expansion of the gelatinous covering, immediately after the egg is placed in water, and the susceptibility of the egg to become impregnated; and that in proportion as the covering becomes enlarged and distended by imbibition of water, the susceptibility of the egg becomes diminished; until at the end of about half an hour it is almost completely lost, at which time the rate of expansion of the envelope is also greatly lessened, and the envelope itself has attained to more than two-thirds its future diameter. Now SPALLANZANI found that the susceptibility of the ovum, when immersed in water, had ceased at the end of fifteen minutes, at which time the envelope is considerably enlarged. PREVOST and DUMAS also observed that the expansion of the envelope is greatest during the first three hours, and rightly regarded the occurrence as connected with impregnation, and with this opinion made experiments to test their views. They placed ova taken from the oviducts in ink, and found the envelopes blackened with the imbibed fluid;* but they remark—"bientôt cette imbibition s'est arrêtée à cause de la reaction chimique de l'encre qui coagulait la matière muqueuse." Afterwards they employed the blood of the Frog mixed with water, and found the envelope deeply reddened when immersed in it; and thence concluded that the envelope in its normal condition admits of the entrance into it of solid particles of matter held in suspension in the fluid. But each of these experiments appears to be open to a different explanation. It is probable that the chemical action of the ink, by altering the condition of the envelope, allowed of the admission into it of solid particles only in proportion to the change in the tissue; and that the colour given to the envelope by frog's blood was due to particles of colouring matter which adhered to the surface, rather than to the admission of these into the substance of the tissue. This conclusion is founded on the following trials.

The imbibition of water by the covering of the egg being so distinctly marked, I had intended, like the authors mentioned, to endeavour to ascertain whether coloured water could be as readily absorbed as pure water. Through accident, however, I omitted to put this question to the test until late in the season of last year, and after the whole of my frogs had spawned. But having placed several ova in rectified spirit for future examination, at the moment of passing them from the body of a frog, I determined to test the result of the immersion of these in coloured fluid, although well aware of the correct objection that would be made, that the experiment must of necessity be inconclusive. I put some of these into a solution of carmine in water, and watched the result. The envelope, which, while the egg was in spirit, was white, opaque, and adhered closely around the yelk membrane, began to imbibe and expand the instant it was placed in the solution ; and at the expiration of an hour there seemed reason to believe that the trial had succeeded. The envelope was much enlarged, and the fluid had penetrated into it, carrying with it some colouring matter; as on carefully washing the surface repeatedly in clean water, to remove the deposit on the exterior, the substance of the interior was seen to be coloured, and it was evident that the colouring matter had penetrated as far as the deepest or thick layer of the envelope. This result appeared to favour the view that the spermatozoon enters the ovum. When the ova were again examined at the end of the third day's immersion, they were of a deep red ; the deepest stratum of colour being then between the vitelline membrane and the thick or innermost layer of the envelope. The entire egg presented the appearance of a globule or bead of red glass, with a dark red centre, surrounded by a lighter-coloured halo. The eggs were then thoroughly washed and placed in clean water. At the end of six hours part of the colouring matter had again been removed from the interior, and the eggs were of a less deep hue, and the water they had been immersed in was coloured. I then again removed them to clear water. At the end of sixteen hours they had parted with more colour, but were still red, more especially between the inner portion of the envelope and the vitelline membrane, and the water had again acquired a red hue, thus shewing that both endosmose and exosmose must have taken place.

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* *Loc. cit.*, ii. p. 132.

As it might fairly be objected that these ova, changed by immersion in spirit, were unfitted for experiment, I made trial with others which had not been impregnated, and being infertile had remained in water many days without giving signs of decay. When these were placed in the carmine solution, their envelopes became as deeply and thoroughly imbued with colouring matter throughout their whole substance as in the former; and when placed in clear water they parted as readily with a portion of it, so that it was evident, that whenever the density of the fluid in which these dead and infertile ova were immersed, was altered, a change by endosmose or exosmose immediately took place in the fluid retained mechanically in their tissues. To this cause, perhaps, may be ascribed the colouring of the ova in PREVOST and DUMAS's first experiment with ink, while other experiments, which I shall mention, made on living and impregnated ova, lead me to regard the colour in the experiment with frog's blood as merely the result of adhesion of colouring matter to the surface.

The immediate objects I had now in view were, to learn whether impregnation is effected by any direct and palpable infiltration of seminal matter through the envelopes of the ovum;—whether the admixture of other matters with the seminal fluid will prevent or arrest impregnation;—and whether the spermatozoa collected on a filter paper, and then placed with this in a fluid of great density, are as efficient as in clear water.

With these views, I prepared a very dense solution of carmine pigment in water, and added parts of this to small quantities of water with ova, either before the seminal fluid was mixed with the water, or immediately afterwards, and I expected the results to shew whether any solid particles, held in suspension in the fluid, passed through the envelopes. The previous trials had shewn that solid particles do pass through the *dead* tissue, but it was doubtful whether the like result would occur in the living.

Carmine Experiments.—Set O. March 13, 1850. Atmosphere 53° Fahr.

No. 1. *Eleven unimpregnated ova* were passed into water mixed with carmine.

The envelopes became as fully expanded, and imbibed fluid as freely as in the impregnated ova, and acquired a red tint; but much of the colour was due to the deposition of granules of matter on the surface, while I was unable to detect any similar granules within their texture. On the contrary, on removing part of the surface of the envelopes, the interior, although slightly reddened, exhibited a uniform appearance.

No. 2. *Thirty ova* were passed into water that had been mixed with seminal fluid, and immediately *afterwards* a solution of carmine was also added.

Twenty-six of these ova became impregnated and produced embryos; thus shewing that impregnation takes place very quickly, and is not prevented by the addition of a dense colouring fluid, added *after contact* with the impregnating fluid.

No. 3. *Forty-one ova* were passed into a solution of carmine in water which had been mixed with seminal fluid immediately *before* the passing of the ova.

Thirty-three of these ova also produced embryos. It was evident, therefore, that when seminal fluid is freely mixed with a dense medium that holds solid particles of matter in suspension, the spermatozoa are not necessarily prevented from effecting impregnation of the ova. Thus the ova of the

Frog, although usually deposited in slow-running or clear still water, may be deposited even in slightly turbid water without impediment to the natural process of impregnation, as the water and spermatozoa may be brought into contact with the ova at the same instant.

No. 4. *Thirty ova* were passed into water mixed with fluid that had been almost completely deprived of spermatozoa by filtration.

Only *one ovum* exhibited any signs of impregnation, but not a single embryo was produced.

No. 5. About *two hundred and twelve ova* were passed into a dense solution of carmine and water in which the filter paper with spermatozoa, separated from the fluid employed in No. 4, had already been placed, and the water and ova were then freely agitated together.

The result of this experiment was very marked. Only a few of these ova became segmented, and the change proceeded much slower in them than in the ova of experiments Nos. 2 and 3. At the end of twelve days only *five embryos* had been produced. Thus a dense solution of carmine, applied to the spermatozoa *before* they are brought into contact with ova, may have the effect of preventing impregnation, apparently by operating as a mechanical impediment. These ova, excepting only a few removed for the following experiment, No. 6, which were taken from the mass as stated, were allowed to remain in the carmine for twenty-four hours before they were placed in clear water.

No. 6. *Forty ova* taken from the last experiment were removed to clear water at the end of one hour and a quarter, having first been thoroughly washed. The result was as decided as in No. 5. *Only two embryos* were formed ; so that there was further reason to believe that impregnation takes place very quickly, and that the result in No. 5 was not entirely due to long continuance in the solution, but to some impediment at the time of contact.

No. 7. A thick solution of carmine was mixed with seminal fluid and water, and *three minutes afterwards* a mass of ova were passed into it.

This experiment was similar to No. 3, excepting only that the solution of carmine was much more dense, and the ova were not passed until three minutes after the fluids had been mixed. There was a marked difference in the result. Only a few of these eggs became segmented, and only *eight* out of a large mass produced embryos. But there were several eggs that appeared to have been *partially impregnated*, the whole of which were abortive. *Partial impregnation*, as before stated, is shewn in a very imperfect cleavage of the yelk, sometimes on one surface only, and sometimes complete as regards one-half of the yelk, but imperfect or irregular in the other.

No. 8. *Forty-two ova* from the last experiment, No. 7, were removed from the carmine at the end of *thirty seconds*, and were immediately well washed to get rid of the adhering spermatozoa and granules of colouring matter. Not one of these ova produced an embryo, but several had become *partially impregnated.*

These experiments were made in the middle of March, when the season was unusually cold, and the mean lowest temperature of the room during twelve days was only 43° Fahr., and the mean highest 47° Fahr.; at a higher temperature the results, I have little doubt, would have been more favourable. The eggs had been impregnated however when the temperature, during the first twelve hours, was 55° Fahr., so that the question respecting the infiltration of solid matter with the water absorbed by the envelopes of the eggs was not affected.

About *four hundred and twenty* eggs were employed in this set of experiments; yet I could not detect any granules of the colouring matter of carmine held in suspension, and of dimensions equal to those of the spermatozoa, which had passed into the tissue of the envelopes of the eggs, although they had become tinged by the colouring matter in combination with the water. Abundance of granules of colouring matter of most minute size, and not more than one-third the diameter of the spermatozoa of the Frog seen beside them, adhered to the surface of the envelopes, and it was to these chiefly that the red colour of the whole was due. Every part of the envelope exhibited the same uniform appearance, the granules being pretty equally distributed over the surface, and the suffusion of colour was uniform in the interior. These facts appeared to be conclusive with reference to the question of the presumed existence of a fissure or perforation through the coverings of the egg of the Frog before, or at the moment of fecundation, as is supposed to exist in the ovum of the Rabbit. I have not been able to detect any appearance of orifice or fissure in the egg of the Frog-envelopes, and the course of which, if such really exists, would no doubt be indicated by some deposition of the colouring matter of the carmine, to a greater or less extent, in its track. The result of these experiments was thus most unfavourable to the belief that the spermatozoa penetrate bodily through the membranes of the ovum; and to that of the supposed existence of a special opening in these membranes for their admission.

I ought now, however, to mention one experiment that seemed to favour the opinion that the spermatozoon enters the ovum. I had taken several ova, together with the oviducts into which they had passed, from the body of a *Lissotriton palmipes*, and others from that of *Triton palustris*, for the purpose of artificial impregnation. Some of these I pressed from the oviducts into a very clear solution of carmine, taken from a solution which had remained undisturbed for nearly a fortnight, so that the granules in suspension had subsided, and only the colouring matter actually combined with the water gave it its red hue. At the end of half an hour I removed the ova from the solution to clear water for examination, and then found that the interior of the envelope was coloured by the water which had entered, but that the greater portion of the colouring matter had been arrested and separated at its entrance and adhered to the surface. One ovum of *Lissotriton palmipes*, however, to my great surprise, had a little dense mass of colour deposited at one point only, of the dark surface of the ovum, not merely within the envelope or its chamber, but actually beneath the vitelline membrane, between it and the yelk, as was distinctly proved by turning the egg on one side and viewing it in profile. Not one of the other eggs placed in the solution, either of the *Triton* or *Lissotriton*, shewed any appearance like this; so that while I am debarred from expressing a decided opinion that the spermatozoon does not enter the ovum, I can only regard the appearance mentioned as entirely accidental, and not as a normal occurrence; but as resulting, perhaps, from some minute puncture or other accident during the removal of the eggs from the body or the oviduct.

But in order, if possible, to remove another source of doubt, it seemed necessary to make some trial with the colouring material employed by PREVOST and DUMAS in their experiments; and some further examination of that used in my own; and to ascertain whether any solid particles or granules of matter, held in suspension in ink or in carmine, and equal in size to the spermatozoa of the Frog or the Newt, can be passed through the filter, or can be separated from the fluid portion by filtration, like the spermatozoa, when precisely the same mode is followed, and the same means and same description and number of filter-papers are employed, as in the filtration of the seminal fluid. The solution of these questions it was evident must tend to confirm or to unsettle the previous conclusions. I first tried *ink*, and used a part of the identical filtering-paper employed to separate the spermatozoa. The ink passed quickly and freely through three filters without losing any of its intense black colour, and carried with it only a very few extremely minute granules, much smaller in size than the spermatozoa of the Frog; so that it seemed fair to conclude that the colour imbibed by the ova from *ink*, in MM. Prevost and Dumas' experiment, was due to the admission of the chemically combined colours of the fluid, and not to an admission into the texture of the egg-envelopes of solid particles held merely in suspension in the fluid. Consequently this experiment seemed to negative the supposition that, from the fact of the interior of the egg-covering becoming blackened, solid particles of matter, equal in size to the spermatozoa, must have penetrated into the envelope during its expansion; and there seemed less reason to believe that the spermatozoa,—bodies very much larger than the ink-granules,—could enter it.

Carmine was then tried. A solution of this colouring matter could scarcely be made to pass through even a single filter. This seemed to be due chiefly to the fact that the greater proportion of the colouring matter of the carmine used (the water colour pigment of artists) was combined with gum and an earthy base, and consequently most of the colour was in suspension rather than in chemical combination. When placed on a single filter, the solution passed through with extreme difficulty and slowness. When a microscopic drop of the fluid so passed was examined with a power of three hundred diameters, it was found to contain a large quantity of granules suspended in clear fluid. When made to pass, but with still greater difficulty, through a second filter, it still contained a quantity of minute granules, but each *less than one half the diameter* of the spermatozoon. It is *possible*, therefore, that some extremely minute granules may penetrate into the texture of the envelope, formed as it is of aggregations of cells; but it seems to be very improbable that any of the larger-sized objects, such as the spermatozoa, can enter: and it is even much more improbable, that if the chief colour of the ova in my experiments was due, as I believe, to granules of carmine on the surface, and not in the interior of the ova, that in MM. Prevost and Dumas' experiments with frog's blood, the ova should have become reddened by the admission of particles of this into their interior, since it need scarcely be mentioned that the colour of the blood is due only to the particles suspended in it; and MM. Prevost and Dumas remark, that they were not able to detect any blood-globules on the surface. To what else, then, than to these, or to their broken-down particles, could the reddened colour of the ova in their experiments be due?

The conclusion, then, to which I am led by these experiments is, that although the envelopes of the egg imbibe coloured fluid, they do so less easily than when the fluid is not coloured, unless it is in chemical combination; and although atoms of solid matter, very much smaller than the spermatozoa, may possibly be carried by infiltration into the texture of the egg-envelope by the act of endosmose during its expansion, it appears to be extremely unlikely that the large bodies of the spermatozoa are so carried in; an improbability which is raised almost to a certainty by the fact that the spermatozoa are not seen attached to the egg with a centripetal direction of the axis of their bodies, but are constantly applied laterally to, or are entangled amongst the loose tissue of the surface, extended at length or partially folded on themselves.

6. AGENCY OF SPERMATOZOA AS AFFECTED BY CHEMICAL MEDIA.

The experiments with carmine having led to an unexpected result in the impediment which this medium offers to the impregnation of the ovum when immersed in it before contact with the spermatozoa, I was desirous of ascertaining what effect would be produced on the ovum by the destruction of the spermatozoa by chemical means, immediately after they had been applied to it. Mr GULLIVER* long ago shewed that the spermatozoa of different animals are variously affected by different chemical tests; and Dr FRERICHS,† more recently, has found that a solution of caustic potass has the property of entirely dissolving and destroying them. This material, therefore, seemed to be peculiarly fitted for the object in view. But before any experiment in which this was employed could be relied on, it was necessary to confirm the facts ascertained by chemical investigation, by observing the mode of action of solutions of caustic potass, and other chemical agents on the spermatozoa, by means of the microscope. As my observations on the effect of chemical agents on the spermatozoa have been confined for the present to those of the Frog, I shall state the results of these observations with the microscope before mentioning the experiments.

All the observations were made immediately after the spermatozoa employed had been obtained, by the course already mentioned, and not by vivisection from the *vesiculæ seminales* or the testes, sources which are objectionable from the facts shewn in Dr FRERICHS' analysis, that a large quantity of albumen is always found in the immature cells in the testes, with which the spermatozoa, obtained from that source, are constantly mixed, while there is no trace of albumen in the mature spermatozoa.

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* Proceedings of the Zoological Society, part 10, p. 101. July 26, 1842.

† In Cyclopædia of Anatomy and Physiology, Article "Semen," p. 506, January 1849.

1. *Solution of Caustic Potass.*—The solution employed was in the proportion of twenty grains of caustic potass (*Potassa fusa*) to one ounce of water. This was the solution employed on most occasions in the following experiments, and which quickly and entirely dissolves the spermatozoa. When a drop of semen, in which the spermatozoa are active and abundant, covered by a pellicle of talc on a plate of glass, is attentively examined, while a very small quantity of the potass solution is applied to the edge of the talc, the act of dissolution is easily witnessed. As the solution spreads beneath the talc the spermatozoa first brought into contact with it are instantly destroyed, while the motions of those at a distance became slower and slower, until, when the fluid has nearly approached, they entirely cease. The instant the fluid comes into contact with the spermatozoa, they roll up on a sudden into a spiral form, the change commencing at the apex of the caudal extremity, and each becomes a rounded mass, which quickly dissolves and disappears in the homogeneous fluid. The action of the potass in this destruction of the spermatozoa, as seen by the microscope, is very similar to, in appearance, and strongly reminds one of the action of fire on the barbs of a feather, which become frizzled in an instant, leaving only a scoria that soon disappears.

2. *Nitrate of Potass.*—This, as in the preceding case, was in solution in the proportion of twenty grains to an ounce of water. It destroys the spermatozoa much less quickly than the caustic potass. When applied, as above, to the edge of the talc, the spermatozoa first become on a sudden motionless, and are in general elongated, and afterwards are very slowly dissolved.

3. *Diluted Acetic Acid.*—When this is applied to the spermatozoa in the same way as the solution of potass, it quickly destroys all signs of vitality. The movements immediately become slower and very soon entirely cease, and the spermatozoa are extended at full length, and are but rarely folded on themselves, as they usually are in natural death. I could not satisfy myself that the acid has any other effect on them chemically than that of contracting and rendering them

smaller. It did not appear to dissolve them. Mr GULLIVER* has mentioned that the spermatozoa of the snake (*Natrix torquata*) are not affected by acetic acid, but he makes no reference to its action on those of the Frog.

4. *Gum Arabic.*—A thick solution of gum appears to act on the spermatozoa mechanically only, and almost immediately deprives them of motion by the obstruction it opposes to them. When a minute drop of spermatic fluid is placed in the midst of one of gum solution, and covered with talc, those spermatozoa which have become mixed with the gum cease to move instantly, and remain with the tail and body coiled in various directions; while others at the edges and in the midst of the fluid, where they are less mixed with gum, still move feebly for a few seconds, but become motionless as the gum collects around them.

These circumstances will better enable us to understand the following experiments, the object of which was to endeavour to learn how far the influence of the spermatozoa, and the *act* of impregnation of the ovum partake of a chemical or of a mechanical nature; and also will help to determine the length of period of contact requisite for impregnation.

The following experiments bear on these inquiries:—

Potass experiments.—*Set* P. March 25, 1850. Atmosphere 48° Fahr. Water 46° Fahr.

No. 1. P.M. 1^h 40^m.—*Fifty-four ova* were passed from the Frog on a dry surface, and were instantly bathed with recently-obtained impregnating fluid mixed with water; and at the lapse of *fifteen seconds* were washed by means of a hair-pencil loaded with the solution of caustic potass before mentioned; after which the eggs were again washed freely with water.

No. 2. P.M. 1^h 45^m.—*Thirty-nine ova* were treated in precisely the same way, except that the interval between the application of impregnating fluid and the solution of potass was only *five seconds*.

The ova were removed, after the first day, to a room in which the average temperature was about 60° Fahr., and at the end of the *eighth day twenty-one embryos*, advanced to near the end of the *fourth period* of development, had been produced in No. 1, and *two embryos*, at a similar stage in No. 2. This, at first thought, appeared to be an extraordinary fact, seeing that the solution of potass so quickly decomposes the spermatozoa, and even renders the ovum steril, as was afterwards found; but, on examination of the details, the experiments admit of explanation:—*first*, the seminal fluid was employed immediately it was obtained, and *before* the application of the potass, which was not used, in the first case, until after a lapse of *fifteen seconds*, and in the second case, of *five seconds;* next, that in both instances the solution of potass was as quickly as possible after its application, diluted and removed by repeated washing of the ova with water. Nevertheless, these experiments prove that the ovum becomes impregnated very quickly after the application of spermatozoa, and, in these cases, even within the short interval of *fifteen seconds* in the one, and *five seconds* in the other, the difference in the number of embryos produced in the two apparently indicating the extent in each of the deleterious effect of the solution.

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* *Loc. cit.*

No. 3. P.M. 1h 50m.—*Forty-seven ova* were passed on a dry surface, and spermatic fluid was instantly applied to them, and within *ten seconds* afterwards the ova were washed with the solution of potass, which was allowed to remain, and water was then added. At the end of the *eighth day* not a single embryo had been produced. The difference between this experiment and the above was the non-removal of the potass, and the more free bathing of the ova.

No. 4. P.M. 1h 53m.—*Forty-five ova* were passed on a dry surface, and were *first* bathed with solution of potass, and then with seminal fluid in water, and afterwards they were removed with No. 1 and 2, to higher temperature.

At the expiration of the *eighth* day *three embryos* had been formed. This result at first appeared to be more difficult of explanation than the former. But, when the circumstances are considered, it seems to admit of quite as easy an interpretation. The bathing of the ova freely with seminal fluid mixed with water, *five seconds after* the application of the solution of potass, diluted this solution too much to allow of its effect on the whole of the spermatozoa applied to the ova, while this very dilution enabled the impregnating influence of these bodies to take effect in some of the ova. The fact, however, leads to an inference of some importance with reference to the action of the potass on the envelopes of the ovum, and seems to shew that this action is less immediate on the envelopes than on the spermatozoa.

No. 5. *Fifty-nine ova* were bathed with seminal fluid and water, and *five seconds afterwards* with a solution of nitrate of potass (in the proportion of twenty grains of the salt to one ounce of water), and water was then added to them.

No. 6. *Seventy-two ova* were treated in precisely the same way.

Not one embryo was produced in either of these experiments.

No. 7. *Forty-four ova* were washed with diluted *acetic acid* immediately after they had been shed from the female, and *five seconds afterwards*, seminal fluid with water was added to them.

No. 8. *Seventy-six ova* were washed with diluted acid, and treated in every way as in No. 7.

Not a single embryo was produced in either of these trials.

The action of acetic acid is almost instantaneous on the envelope of the ovum, which it quickly contracts, and renders slightly opaque.

Besides those media which act *chemically* on the spermatozoa and the ova, I made trial, in this and the two following sets of experiments, with a solution of gum-arabic, the effect of which appears to be entirely *mechanical;* and as the results are curious and seemingly important with reference to the nature of the agency of the spermatozoa in impregnation, I defer the mentioning of them until I have to shew the effect of media which operate mechanically on the ovum or the spermatozoon in impregnation.

The result of the preceding experiments was so remarkable, that it seemed necessary that they should be repeated with greater precision, with reference to exact periods of time, than can always be done when alone and unassisted. It was evident that a proper understanding of the nature of the act of impregnation, if ever this becomes known, will be led to chiefly by attention to the *periods of time* in which it is effected. I obtained therefore the assistance of a friend, to note the spaces of time which elapsed in each stage of the following experiments, so that these might be performed with the quickest despatch, and the attention of the experimenter be not withdrawn from each until it was completed.

Set Q. March 30, 1850. Atmosphere 49° Fahr.

The seminal fluid employed in this set of experiments was not obtained from the usual source. From some cause or other it could not be so procured. I therefore killed a male frog, by dividing the spinal cord in the *medulla oblongata*, and pressed out the fluid from the testes, which were gorged with spermatozoa, as found by examination with the microscope. From the presence of a great number of spermatozoal cells, and from the water with which I mixed the fluid becoming slightly turbid and albuminous, it was seen that it was not fully matured, a circumstance to be borne in mind with reference to the experiments, which were commenced some minutes after the fluid had been thus obtained. It was doubtful also whether the eggs were quite mature.

It is necessary further to mention, that in these experiments two solutions of potass, with different proportions of the salt, were employed; one having twenty grains in one ounce of water, and which, to avoid repetitions, I shall designate *strong solution;* and the other having only eight grains of the salt in one ounce of the fluid, and which I shall refer to as the *weak solution.*

No. 1. P.M. 4h 45m.—*Forty-five ova,* passed on a dry surface, were bathed with seminal fluid and water, and *five seconds afterwards* with the *weak solution* of potass, and were then washed, and placed in clean water. The whole time occupied in the experiment did not exceed *thirty-five seconds.*

On the following morning, segmentation was found to have taken place in *twelve ova.* This as well as the following sets of ova were then removed to a higher temperature, in which they were allowed to remain; but *no embryos* were produced.

No. 2. P.M. 4h 58m.—*Fifty-three ova* were treated in exactly the same way, but with the *strong solution* of potass; the *interval* between the application of the impregnating fluid and the solution being only *two seconds,* and the whole time occupied *forty-five seconds.*

Segmentation took place in *three or four ova,* but not completely. Several ova also were altered in form; but *not a single embryo* was produced.

No 3. P.M. 4h 52m.—*One hundred and twenty-two ova,* passed on a dry surface, were immediately well bathed with seminal fluid and water, and *two seconds afterwards* with *weak solution* of potass, which was *allowed to remain with the ova,* and water was then added. The whole time occupied was *forty-five seconds.*

Segmentation took place in a great number of these ova, certainly from *fifty to sixty,* and there were nine also which became shrivelled and decayed. Notwithstanding the great proportion of ova segmented, *not one produced an embryo.*

No. 4. P.M. 5h.—*Sixty-six ova,* passed on a dry surface, were bathed as in No. 2, with impregnating fluid, and *one second afterwards* were washed quickly with *strong solution* of potass, and water was then added; the period occupied being *sixty-two seconds.*

Only *two ova* became segmented, while, excepting only eight or nine ova, the whole of the remainder were shrivelled, irregular, or compressed in form, and distinctly spoiled, apparently by the action of the potass, thus shewing that endosmose and exosmose through the envelopes had taken place.

No. 5. P.M. 4h 55m.—*Fifty-four ova* were passed, and washed with the *weak solution* of potass, and *afterwards,* at an interval of *two seconds*, with impregnating fluid and water, and water was then added and allowed to remain. The whole time occupied was *fifty seconds.*

One ovum only became segmented; but several others had slightly altered their form to the obtuse oval, as if about to become divided: *no embryo* was produced.

No. 6. P.M. 5^h 2^m.—*Seventy-nine ova* were washed with *strong solution* of potass, and one *second* afterwards with impregnating fluid, and water was then added.

Not one ovum became segmented, nor did even one yelk retain its proper shape. The whole were irregular and spoiled. *Five ova* had the envelope clouded and opaque, and the surface of others was translucent with refracted light, like crystallised carbonate of lime. In one egg only had there been any attempt at segmentation. This experiment, like No. 4, seemed to shew that the act of expansion of the chorion is an act of endosmose.

No. 7. P.M. 5^h 15^m.—*Nitrate of potass. Seventy-seven ova* were passed on a dry surface, and were well bathed with impregnating fluid in water, and *one second* afterwards with a *weak* solution of nitrate of potass, and water was then added to them; the whole time occupied being *thirty seconds.*

Segmentation took place in *forty-three* of these ova, and the whole retained their natural form and size. The effect of the nitrate of potass, as compared with the caustic solution, was thus very marked, as shewing that the momentary application of the nitrate does not prevent or arrest impregnation in weak solution (eight grains to one ounce of water), even when applied *after* the impregnating influence. The experiment was also interesting in another respect. It proved that the ova were susceptible of being impregnated, and that the fluid from the testes was efficient to induce the first evidences of impregnation. But none of these ova, or of the ova in the preceding experiments of this set, produced embryos. Subsequent observations will shew that this failure was not due to the nitrate of potass, but perhaps was attributable to the conjoint causes of low temperature at the time of impregnation, and of some imperfection both in the seminal fluid and the ova.

No. 8. P.M. 5^h 9^m.—*Seventy-six ova* were washed with diluted *acetic acid* as in *Set* P. No. 7, and *one second* afterwards with impregnating fluid, and water was then added to them. The time occupied was *forty-five seconds.*

The result was more decided than in the experiment referred to. The envelopes of the ova immediately became clouded, and no segmentation took place in any of the yelks, some of which became shrivelled and changed in form.

The result of the preceding experiments being doubtful as to the cause of the non-production of embryos, especially with reference to the first three, and the seventh experiments, in which many ova became segmented, I obtained some additional pairs of frogs from their native haunts, and within twenty-four hours afterwards, before they had in any way become debilitated by confinement, repeated the experiments at a higher temperature.

Set R. April 3, 1850. Atmosphere 60° Fahr.

No. 1. P.M. $3^h 5^m$.—*Eighty-two ova*, passed on a dry surface, were touched for *an instant only* with a pencil dipped in impregnating fluid and water, and *one second afterwards* were washed with *strong* solution of potass, and then with water, and water was then added. The whole time occupied was only *fifteen seconds*.

No. 2. *Twenty-five ova* were treated precisely in the same way with the same solution (which also was employed in the following experiments); the interval being *two seconds*, and the whole time *twenty seconds*.

Segmentation took place, but *only very partially*, in about *twelve ova* of the first, but completely in *one only* of the second experiment. The whole of the remaining ova were shrivelled and decayed; their envelopes exhibiting the same clouded and refractive property noticed in the last set. At the end of five days *two embryos* had been produced in the first experiment, and *one* in the second. It was thus far confirmatory of the experiments with potass in *Set* P. that if this salt be applied to the envelope *several seconds after* the application of the impregnating fluid, and be again *quickly removed or diluted* with water, impregnation may already have taken place, and the action of the caustic will not in that case affect the production of the embryo; especially if the experiment be made when the temperature of the surrounding medium is becoming increased. But if the solution be applied *before* the application of the seminal fluid, then the spermatozoa will, in most cases, be decomposed, and no impregnation follow. In either case, however, the undiluted solution acts also on the ovum itself within a very short period, and destroys or renders it steril. This was further proved in the succeeding experiments.

No. 3. P.M. 3^h 10^m.—*Fifty-eight ova* were passed on *a moistened surface*, and were immediately afterwards washed with the solution, and at the expiration of *one second* were bathed with seminal fluid and water; the time occupied being only *fifteen seconds*, as in No. 1.

No. 4. *Sixty-nine ova* were treated in exactly the same way, the interval being *one second;* and the whole time occupied only *twelve.*

Partial segmentation had taken place in *one ovum* of No. 3; but the whole of the remaining ova, both in Nos. 3 and 4, were destroyed. Many of the yelks had begun to change form within the first hour, and the envelopes exhibited the same refractive appearance as in the previous experiments.

Anticipating from the former experiments what probably might be the ultimate result in these, I now determined to put beyond the possibility of doubt, both the fitness of the seminal fluid employed to effect impregnation and the healthiness of the ova, and their susceptibility to become impregnated; and to shew from these facts that a non-production of the embryo *in this set of experiments* must be due to the action of the potass solution, and not to any unfitness in the spermatozoa or the ova. Accordingly,—

No. 5. P.M. 3^h 17^m.—*Sixty-two ova*, from the same female employed in the preceding experiments, were bathed with a portion of the seminal fluid and water which had been employed in Nos. 1 and 2, and were then placed side by side with these, in a separate dish.

At the expiration of *four hours and thirteen minutes*, the temperature being 60° Fahr., from *thirty to forty* of these ova had become segmented. Some of the ova had been injured mechanically, but nearly the whole that had not been injured were impregnated. On the *seventh day* there were *twenty-three embryos*, *thirteen* of which had already left the egg-envelopes; others were somewhat less advanced, thus proving the fitness of the seminal fluid to impregnate, and the ova to produce. The number of embryos too was fully as great as could have been expected, seeing that many of the ova had been slightly injured, and that the seminal fluid had already been *one hour and twenty-six minutes* mixed with water.

The result of this experiment was borne out by the following.

No. 6. P.M. 3^h 31^m.—*Nitrate of Potass. Seventy-four ova* were well bathed with impregnating fluid and water on a previously dry surface, and *one second* afterwards with a *strong solution* of nitrate of potass (twenty grains to one ounce of water), and water was then added to them; the whole time of the experiment being *twenty seconds.*

No. 7. *Fifty-nine ova* were treated in exactly the same way, save that the interval between the application of the impregnating fluid and the solution of potass was *three seconds*, and the whole period *twenty-five seconds*.

Segmentation commenced in each of these sets in *four hours and fourteen minutes*, when from *twelve* to *fifteen* ova were undergoing this change in No. 6, and *thirteen* in No. 7. At a later hour there were many more in each experiment only very partially segmented, and which proved to be unproductive. At the end of the *seventh day*, there were *twelve embryos* in No. 6 advanced to the same stage as in the *simply artificial impregnation* No. 5, and *ten embryos* in No. 7, but at a little less early stage of growth, a circumstance which I attributed at the time to imperfect aëration.

No. 8. P.M. 3h 37m.—*Seventy-nine ova* were bathed with the same solution of nitrate of potass as above, and *three seconds afterwards* with impregnating fluid in water; the whole time occupied being *twenty seconds*.

Segmentation took place at a little later period in this than in the preceding experiments. It commenced at *four hours and twenty minutes*, when *twenty-five ova* were undergoing the change. This was a full proportion of impregnation as compared with No. 5, seeing that the impregnating fluid had already been mixed with water one hour and forty-six minutes. *Twenty-five embryos* were the result of this experiment.

The results thus support the explanation already given, with reference to the effect produced on the envelopes of the ovum being less immediate than on the spermatozoa; since, in this case, twice as many ova became segmented, and ultimately produced embryos, as in those experiments in which *the solution* was applied *after* the seminal fluid, and while endosmosis of the egg was most rapid, and when the solution remained undiluted.

The general results of this set of experiments, compared with those of the last *Set* Q. appear also to shew that the non-production of embryos in the whole of that set,—after segmentation had taken place in several of the experiments, as in Nos. 1, 2, and especially No. 3, with solutions of the caustic potass; and still further No. 7 with the *nitrate*,—may fairly be attributed to some defect in the seminal fluid or in the ova; since, if such were not the cause, and the failure had been due either to the chemical effect of the media on the ova, or to the moderate temperature of the atmosphere (49° Fahr.) at the time of experiment,—segmentation of the yelk would hardly have taken place. This supposition appears to be the more likely, when we recollect that in the *Set* Q. and *in that set only*, the impregnating fluid was obtained from the testes, compressed and broken down in water,—that the eggs were of doubtful maturity,—and that this was *the only set of experiments in which no embryos were ultimately produced;* although, I may now mention, that greater care was taken to ensure a favourable result than in most of these investigations,—the ova being removed at the end of twenty hours to an average temperature of 60° Fahr. were retained in flat shallow dishes,—and had the water changed daily. Both sets, however, Q. and R. seem to prove that the *act* of impregnation, as evidenced in the fact of the yelk becoming segmented, must take place, or be commenced very rapidly; and, apparently, *almost at the instant of contact of the spermatozoon with the coverings of the ovum;* as seems to be shewn in the fact, that segmentation took place in many of the ova when the space of time between the application of the spermatozoon, and that of the solution,—which previous observation (p. 28) shewed was sufficient to decompose it immediately,—was scarcely more than *one* or *two seconds.* Thus in Q. 4 and R. 1, it must have commenced in the interval of *one second,* even when the strong solution was used; and in Q. 2 and R. 2 with the same solution in *two seconds.* When the weaker solution was used, a greater number of ova became affected in similar spaces of time, as in Q. 2 and 3. These experiments seem to shew that the *act* of impregnation had already been *commenced* before the application of the solution; as, in the experiments which are the converse of those now mentioned, in regard to the time when the spermatozoa and the solution were applied, a different result ensued. Thus, when the *solution* was applied to the ovum *first* as in Q. 6 and R. 3 and 4, and *one second afterwards* the impregnating fluid with spermatozoa, no impregnation, or but a very partial one, was effected. The ova in these three experiments amounted to *two hundred and six,* and yet only *one ovum* became very partially affected. A like result ensued even when the weaker solution was employed at an interval of *two seconds,* as in Q. 5, when out of *fifty-four ova* segmentation occurred but in *one.*

When the interval between the application of the impregnating fluid, in the first instance, and that of the solution subsequently, was extended to *five seconds,* then a greater proportion of ova became segmented, as in Q. 1, with the weak solution, when out of *forty-five* ova *twelve* became changed.

These were the results when the experiments were made at different temperatures, as at 49° Fahr. with the *Set* Q. and 60° Fahr. with the *Set* R. They cannot, therefore, be attributable to inertness of the fecundating agent, or of the object to be fecundated, occasioned by an unfavourable temperature of the surrounding medium. The fact of the occurrence of segmentation in some ova, but not in the majority of the ova of different experiments, as in Q. 1, 2, 3, and 4, seems further to shew that the influence of the momentary application of the potash solution was produced chiefly, and in the first instance, on the spermatozoa, or impregnating bodies, and not so immediately on the ova; since if the ova had been first, or most affected, none of them, probably, would have become impregnated.

Further, I may perhaps be allowed to remark, that the arrest of impregnation was due mainly to the nature of the chemical agent employed; and the extent of interference with the fecundatory process was in proportion to the more or less immediate action of this agent on the spermatozoon. Thus we have seen that but few ova were impregnated when the solutions of caustic potass were employed; but when the nitrate of potass was used as in Q. 7, *forty-three* out of *seventy-seven ova* were segmented; while in R. 6, 7, and 8, in which the total number of ova was *two hundred and twelve*, there were *fifty-three* segmented, and these produced *forty-seven embryos*.

The object of these set of experiments, therefore,—that of endeavouring to ascertain within what period of time after the contact of the spermatozoon with the ovum its fecundatory function is exerted,—appears to have been somewhat fulfilled;—in so far as that in these experiments on the *Amphibia* the *commencement of the act of impregnation appears to have been almost instantaneous*. Yet there seems reason to believe that momentary contact of the impregnating body, even in the ovum of these animals, is not in itself sufficient to complete the fecundation, although it may tend to induce that condition of the yelk, segmentation, which we now are assured is always indicatory of its having been influenced by the fecundatory agent. If momentary contact were sufficient for the completion of the function, then partial impregnation, which so frequently takes place when spermatozoa are few in number, or in contact only for very brief periods, could hardly happen; while every ovum in which the process of cleavage is begun ought to pass through all its changes to the production of the embryo, circumstances being favourable to its development. But this we have seen in the foregoing experiments is not the case. On the contrary, *duration* of at least some *seconds* of contact, varying no doubt in different tribes of animals, and, apparently also, *quantity* of spermatozoa, seem to be essential to fruitful and healthy impregnation, as appears to be shewn in the *filtration experiments, Set* L. 3, as compared with L. 1 (pp. 5 and 6). Possibly momentary contact may suffice to occasion segmentation, but certainly with *duration* of contact the ovum is fecundated.

This leads us further to inquire whether any endosmosis of the material substance of the spermatozoon is imbibed by the ovum during any period of impregnation, before or during segmentation of the yelk?—and whether those media which do not act chemically on the spermatozoon or the ovum can arrest the agency of the former? BISCHOFF has already shewn that spermatozoa are in contact with the ovum in some Mammalia, the Rabbit* and Dog†, from quickly after the entrance of the ovum into the Fallopian tube until segmentation is nearly completed, and the yelk has acquired a tuberculated or mulberry-like surface. In the ovum of the Frog I have shewn that the spermatozoa are in like manner seen on the envelopes from immediately after immersion in impregnating fluid until segmentation has commenced. In the Newt we have seen that *when impregnation is effected artificially*, they may be recognised on the surface for a much longer period,—from the time of contact with fluid, until the surface of the yelk has reacquired its original smoothness, a period, in my observations, of from thirty-six to forty-eight hours. The persistence of these bodies to a period after the first evident changes in the yelk have commenced, seems to favour a supposition that their function is not completed in momentary contact. Although we are at present unable to trace their influence beyond what is now stated, I think it can be shewn that their function can be arrested by media which affect them mechanically, when submitted to such media at the moment of contact with the ovum.

7. AGENCY OF THE SPERMATOZOA AS AFFECTED BY MECHANICAL MEDIA.

The object of the next experiments was to learn whether the interposition of a dense fluid medium, which does not act chemically on the spermatozoa, would be as effectual in preventing the influence of these bodies on the ovum as in the *carmine experiments*, the effect of which seemed to be mechanical.

As the experiments were made at different periods, it will be seen, that although on two of these occasions the temperature of the atmosphere differed, the general results were similar.

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* Entwickelungsgeschichte des Kaninchen-eies. 4to. 1842, tab. 2, 3, and 4, fig. 17 to 28.

† Entwickelungsgeschichte des Hunde-eies. 4to. 1845, tab. 1 and 2, figs. 10 to 16.

Gum and Starch Experiments.—*Set* S. March 25, 1850. Atmosphere 48°.

(*a.*) *Gum.* No. 1. P.M. 2^{h} 22^{m}.—*Seventy-six ova* were passed on a *dry surface* and were immediately bathed with a thick solution of gum-arabic, and *fifteen seconds afterwards* with seminal fluid and water, and fresh water was then added to them. The whole time of the experiment was *sixty seconds.*

No. 2. *Fifty-eight ova* were treated in exactly the same way, the interval being *fifteen seconds*, and the whole time *sixty.*

The seminal fluid employed was obtained from two males, the fluid used to No. 2 being from a male which had paired four days before. Out of the whole number of eggs in the two sets, amounting to one hundred and thirty-four, not one produced an embryo.

No. 3. March 30, 1850. P.M. 5^{h} 5^{m}.—Atmosphere 49° Fahr. *One hundred and eighty ova* were passed on a *moist* surface, and were immediately bathed with a thick solution of gum as above, and one *second afterwards* with seminal fluid in water; the whole time occupied being only *sixty seconds.*

Segmentation took place in *two*, or at most only *three* of these ova, and even in them very imperfectly, and much slower than in the corresponding ova of the set to which they belonged, *Set Q.* (pp. 32 and 33), in which the fluid employed was obtained from the testes of the Frog, and regarded as immature.

No. 4. P.M. 5^{h} 18^{m}.—*Fifty-eight ova* passed on a *moistened* surface were immediately bathed with solution of gum, and one *second afterwards* with seminal fluid from the same male as No. 3. The whole time occupied was *forty-five seconds.*

The result of these two experiments, as compared with others of the set to which they belonged, *Set* Q., was exceedingly curious. In the experiments with the nitrate of potass as in Q. 7, segmentation was carried to some extent, and the divisions of the yelk were multiplied; while only *four ova* out of the fifty-eight, in this with gum, gave any evidence of segmentation, and the process was not advanced further, either in this or in the preceding experiment, No. 3, than to the completion of the primary division of the yelk into two hemispheres. Thus not only was the process entirely prevented in the great majority of the ova, but it was also very much retarded in those in which it did take place, and this simply, as it appeared, by the mechanical hindrance of the gum. Could it be that the effect was produced on the endosmic action of the yelk? These trials certainly appeared to shew that the obstruction was a mechanical one. I need scarcely remark, that *no embryo* was produced in either of these experiments.

No. 5. April 3, 1850. P.M. 3^h 25^m.—Atmosphere 60° Fahr. *Sixty-one ova* were passed on a *moistened* surface, and were immediately bathed *with impregnating fluid*, and *two seconds afterwards* with a thick solution of gum-arabic, and water was then added; the whole time occupied being only *twenty seconds*.

This experiment, when compared either with the four preceding ones, made at a temperature of the atmosphere eleven degrees lower, or with that which follows, No. 6, seems to point to the exact nature of the operation of the gum. At *four hours* and *five minutes* from *fifteen* to *twenty* ova had become segmented, and others were in the act of becoming so. At *seven hours and a half* more than one half of the whole number had changed, and were perfectly healthy. Thus, in this case, in which the gum was applied *after* the seminal fluid, impregnation occurred *earlier* than in corresponding experiments of the same set, R. 6, 7, and 8, with nitrate of potass, when it happened in from *four hours* and *fourteen* to *twenty minutes*. It was even as rapid as in the artificial impregnation, R. 5 (p. 35), in which it took place in *four hours and thirteen minutes*. On the eighth day *twelve embryos* had been produced.

These facts seemed to shew, precisely as in experiments with solutions of potass, that impregnation is commenced very quickly; and further, that it was not arrested by the gum when applied only *two seconds* after the spermatozoa, but that the change proceeds almost as uninterruptedly as in a perfectly natural impregnation, since the number of embryos was almost as great as in No. 5 R., seeing that the fluid employed had been obtained and mixed with water one hour and thirty-four minutes.

No. 6. P.M. 3^h 22^m.—*Seventy ova*, passed on a moistened surface, were bathed with a thick solution of gum, and *two seconds afterwards* with some of the impregnating fluid employed in the last experiment, and water was then added; the whole time occupied, as above, being only *twenty seconds*.

This experiment was the converse of the preceding. At *four hours and twenty-eight minutes* only one egg out of the whole had become segmented; but others gave signs of being about to change, and some hours later a few had done so, but there were not at most more than *ten*. At the end of the *seventh* day *two embryos* had been produced.

Thus while a comparison of these two experiments seems to shew that the gum acts simply as a mechanical obstruction to the process of fecundation, this experiment, No. 6, when compared with Nos. 1 to 4, made at eleven degrees lower temperature, shews also the influence of a higher degree of temperature in accelerating fecundation.

The two following experiments were made with a view to test the efficiency of the fluid and ova employed, now at one hour and fifty minutes after the fluid had been obtained: when examined at this time with the microscope, there were still an abundance of active spermatozoa.

No. 7. P.M. 3h 41m.—*Eighty-five ova* were accordingly placed in water with some of the impregnating fluid and allowed to remain to test its efficacy.

At *four hours and twenty-four minutes* nearly the whole of the ova had become segmented, and on the seventh day *forty-two embryos* had been produced.

No. 8. P.M. 5h 50m.—*One hundred and thirty-two ova* were now passed into the remainder of the impregnating fluid, which had at this time been *four hours* mixed with water.

When examined at the end of five hours and ten minutes, not a single specimen had become impregnated. This was proved by the result, that at the end of seven days *not a single embryo* had been formed. The fluid had thus lost its fecundating property at the end of four hours in a temperature of 60° Fahr.

No. 9. April 6, 1850. Atmosphere 60° Fahr.

P.M. 1h 50m.—One more experiment was now made with the gum solution, for the purpose of comparing it with the following experiments with starch.

One hundred and twenty-two ova were passed on a dry surface and covered with a thick solution of gum, and *three seconds afterwards* with impregnating fluid that had been mixed with water only *thirty minutes*. The time occupied was not noted, but fresh water was added to the ova at the end of fourteen minutes.

Segmentation took place only in two or three ova, at *four hours and twenty-four minutes*, and at the end of ten days only *three embryos* had been produced.

(*b.*) *Starch*. No. 10. P.M. 1h 40m.—*One hundred and fifty-eight ova* were passed into a solution of starch in water, and at the end of *ten seconds* one-half of the whole quantity of seminal fluid, obtained from a single frog, and previously mixed with water, was added to them.

After the ova had remained in the solution and been gently agitated during twenty minutes, they were carefully washed and removed to clear water.

At *four hours and twenty-six minutes* only a very few of these ova, not more than eight or ten, had become segmented, notwithstanding the large quantity of recent impregnating fluid that had been added to them. At the end of ten days only *five embryos* had been produced.

It was remarkable that very few of the ova in this experiment cohered together, as the frog's ova almost invariably do when placed in fluid. On the contrary, most of them remain separate and isolated, although their envelopes had imbibed water and expanded to their usual extent in a similar space of time.

No. 11. P.M. 1^h 40^m.—*Seventy-one ova* were passed on a perfectly dry surface, and immediately afterwards were covered, by means of a hair-pencil, with a thick solution of starch, and at the expiration of *ten seconds* with impregnating fluid, and water was quickly added.

The water was changed at the end of fifteen minutes. At *four hours and twenty-five minutes* only two or three ova had become segmented. At the end of ten days *three embryos* had been produced.

The concluding experiment with starch was the counterpart of No. 5, with *gum*.

No. 12. P.M. 1^h 44^m.—*One hundred and nineteen ova* were passed into water, with which one-eighth part only of the seminal fluid, obtained from the frog, had already been mixed. *Two seconds* afterwards a solution of starch was added to these ova, and at the end of sixteen minutes they were removed to clear water.

In *four hours and twenty-six minutes* segmentation had commenced in many of these ova. The exact number I omitted to ascertain. But the change had taken place in a much shorter space of time, and was more general, although scarcely one-fourth part of the quantity of seminal fluid that had been employed in No. 10 was used in this case. Nevertheless, in ten days *twelve embryos* had been formed.

This experiment, therefore, was quite confirmatory of the conclusions drawn from its counterpart, No. 5, with *gum*, and Nos. 10 and 11 as fully bore out those deduced from Nos. 1 to 5, with the same; while the entire set seem to be in full accordance with the already arrived at conclusion, that fecundation is commenced almost immediately the fecundating body is in contact with the ovum. Thus, then, with regard to the nature of impregnation, we seemed to have obtained sufficient proof that the *act* is effected through the agency of the spermatozoon, and not through that of the *liquor seminis*, as was formerly supposed.

RECAPITULATION AND CONCLUSIONS.

It may be now be well to recapitulate briefly some of the facts and views derived from the foregoing observations and experiments.

First, then, the *germinal vesicle* disappears in the Amphibia before impregnation; and before, or at the time of the bursting of the ovisac, and extrusion of the egg from the ovary into the cavity of the abdomen. It does not return to the centre of the yelk, nor escape to the surface, but is lost much nearer to the latter than to the former position; and its disappearance is the result of the endogenous development of cells in its interior. The egg is cast loose into the abdomen, and then consists only of the yelk mass in its vitelline membrane, and it is transferred to the mouth of the oviduct by the joint action of the abdominal muscles and the motions of the viscera, and not necessarily through the aid of the male during copulation.

Second, Changes are going on in the constituents of the egg, both before and after oviposition as well in the unimpregnated as in the impregnated condition; but they soon cease in the former, and do not proceed to the cleaving or segmentation of the yelk.

Third, That the egg is not susceptible of impregnation until after it has acquired the envelopes which it gains in the oviduct.

Fourth, That endosmosis of the entire egg takes place through these envelopes, and is most rapid during the few minutes the egg is most susceptible of impregnation. Further, that this endosmosis is augmented and hastened by an increase, and is lessened and retarded by a diminution of temperature; and that the susceptibility of the egg to become impregnated, and produce, is in exactly the same condition with regard to heat; whether the egg be exposed to, or whether it be excluded from light.

Fifth, That only extremely minute granules of solid matter can by any possibility pass into the tissue of the envelopes during endosmosis; and that there is no evidence whatever of the existence of a fissure or orifice, in the envelopes of the egg of the Amphibia, at the time of, or before impregnation, capable of admitting the spermatozoon to the interior of the yelk-membrane or its contents.

Sixth, That it is the spermatozoon alone which effects impregnation; and that this does not take place until the spermatozoon is brought into immediate contact with the external envelopes of the ovum.

Seventh, That the *liquor seminis,* when entirely separated from spermatozoa, certainly does not effect impregnation.

Eighth, That although direct contact of the spermatozoa with the ovum is indispensable to effect impregnation, I have never been able to detect any traces of these bodies in contact with the yelk-membrane, or even within the substance of the external envelope.

Ninth, That impregnation is *commenced* the instant the spermatozoa are brought into contact with the egg, but a certain *duration of contact* is essential to its completion.

Tenth, That impregnation is not effected, when the whole or the majority of the spermatozoa in contact with the envelopes have previously become motionless, and apparently have lost vitality, as they are found to have done after the lapse of a longer or shorter period.

Eleventh, That although an exceedingly minute quantity of spermatozoa suffice to impregnate the ovum, the phenomenon of impregnation takes place more tardily, even with duration of contact when the number is extremely limited, than when it is in full abundance, without excess; while, when the quantity is deficient, or the duration of contact too limited, then the phenomenon is incomplete, and partial impregnation only is effected.

Twelfth, Partial impregnation is shewn in imperfect segmentation of the yelk; and is due chiefly to the spermatozoa being insufficient in quantity, or in duration of contact, or inefficient through diminished vitality; and it may also result from diminished susceptibility in the ovum.

Thirteenth, Partial impregnation of the ovum is of frequent occurrence, as I found in my first experiments with fluid that had passed through filtering-paper, but which still contained a very few spermatozoa, either motionless or exceedingly feeble; and further, partial impregnation is of much the most frequent occurrence when the ova are placed in dense fluid before contact with the spermatozoa, as in the experiments with carmine.

Lastly, When the ova are only partially impregnated they are usually, and perhaps always unproductive.

The facts now enumerated lead us to inquire, whether impregnation takes place through any catalytic influence of the spermatozoa, as suggested by BISCHOFF,* while in a state of activity, and at the instant they are brought into contact with the ovum, or whether impregnation results from a diffluence of the spermatozoa thus brought into contact with the surface, the substance into which they may be dissolved being carried by endosmosis with the water imbibed through the tissues; or whether it is the result of the conjoint influence of both these conditions; —the first action induced being instantaneous and catalytic, and possibly dependent on the persistence of organic vitality in the spermatozoa, while the completion of the impregnation may depend on the imbibition of some material influence or substance derived from the impregnating body. This is a view, which the gradual disappearance of the bodies of the spermatozoa from the surface of the ovum, both in the Frog and Newt, seems to favour; as we have already seen that endosmosis is an active and important function of the envelopes of the ovum at the very period when impregnation is effected.

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* MULLER's Archives, 1847.

All the experiments now detailed seem to shew that in those VERTEBRATA which expel their ova into water befoe impregnation, as in the tail-less *Amphibia*, and in which,—from the nature of the medium into which the ova are passed, —we may infer that the function takes place most quickly, impregnation is commenced at the very instant of contact of the spermatozoon with the ovum, and even may be completed within very short spaces of time,—but *duration* of at least some seconds of actual contact,—even in these animals' ova, is essential to the perfection of the function. But this period, we may fairly conclude, may differ in different classes of animals, and possibly may have some relation to the greater or less facility with which the spermatozoa are brought into contact with the ova.

When the experiments last detailed are compared,—the effects produced by the application of media which influence the spermatozoon and the ovum chemically,—with those of which the effect is merely mechanical, we seem to have made some advance towards a future knowledge of the nature of the impregnating power. Although we are as yet entirely without proof, that any material influence or substance is actually transmitted from the spermatozoon on the surface of the ovum to the yelk, in the interior, we have evidence, that fluids are imbibed by the ovum by endosmosis through its tissues; and, although not a trace of the spermatozoon is detected in the interior of the ovum, we have seen that it remains for a long time on the surface, and gradually disappears, apparently by diffluence; so that it may be fair to conclude, that the agency of this body is material in its operation. On the other hand, the effect which we find is produced on the yelk by the direct and even momentary contact of the spermatozoon with the envelopes of the ovum, seems closely to resemble that of the so-called catalytic power of certain known bodies, in so far as that contact, during only very short spaces of time, with the surface of the ovum, appears to be sufficient to induce certain changes in the interior. These changes, too, as known of catalysis, are carried only to a certain extent when the exciting agents,—in this instance the spermatozoa,—are feeble in action or but very few in number; and then, as we have seen, the yelk may become only more or less partially segmented; or the changes in it, having proceeded to a certain extent, may then become arrested, apparently from deficiency of the originally exciting cause.

Then, again, we find that although segmentation of the yelk may take place, embryos are not produced unless there has been some continuance or duration of contact of the impregnating with the impregnated body; and that the number produced seems to have reference to the duration and to the full sufficiency of the exciting cause. But neither what we at present know of the so-called catalytic power or of endosmosis, appears alone to be sufficient to account for the whole of the phenomena of impregnation. Simple contact of the spermatozoon does not appear to be sufficient to determine the transmission of more or less of the material structural characters of the male parent to the offspring; while diffluence and endosmosis of the substance of the spermatozoon can hardly be imagined to occur in a brief second or two of time sufficiently to effect the full impregnation of the yelk, and induce its invariable consequence, segmentation. Possibly, we may hereafter find that the first changes induced by contact of the impregnating body are completed by its diffluence, and by the material constituents into which it is dissolved being transferred to the yelk by endosmosis.

[The facts recorded in this memoir are illustrated by twelve lithographed figures. The descriptions are quite intelligible without these figures. But readers who desire this illustration will find them in the Philosophical Transactions for 1851, Plate Fourteenth.]

ART. II.—*An Account of two Cases, in which Ovules, or their remains, were discovered in the Fallopian Tubes of Unimpregnated Women who had died during the period of Menstruation.* By H. LETHEBY, M.B., London, Lecturer on Chemistry and Medical Jurisprudence in the Medical School of the London Hospital. Communicated by T. B. CURLING, Esq., F.R.S. (Philosophical Transactions of the Royal Society for 1852.—Received February 20. Read May 1, 1851).

[IN 1821 Dr John Power of London published Essays on the Female Economy, a Treatise containing several important observations and original views on the physiological peculiarities of the reproductive organs of the female. In one of these Essays he undertook to explain what he considered to be the true functions of the ovaries, and to demonstrate, so far as it was practicable, the proposition, that, the ovaries are the primary and regulating, governing, or presiding parts of the generative organs. To the influence of these organs he ascribed the peculiar periodical discharge taking place from the uterus. It had been believed that a certain degree of vascular orgasm takes place periodically in the ovaries. This orgasm Dr Power regards as the exciting cause of all the phenomena and actions which take place in the uterine system at puberty, and at every monthly period during the childbearing life of the female. From various phenomena Dr Power inferred that at every monthly period, an ovule or ovarian vesicle is matured and is discharged from the ovary during the flow of the catamenia. He believed that the catamenial secretion is induced in the *uterus* by the ovaries exciting the bloodvessels of that organ; and that the fluid so secreted was an imperfect attempt to form a deciduous membrane.

These ideas and inferences might be right or wrong at the time at which they were published; they might be deduced erroneously or correctly. But little pains were at that time taken to ascertain to what extent they accorded with facts; and it is only after the lapse of between twenty and thirty years that their importance has been recognised by subsequent inquiries. The doctrines of Dr Power were long neglected and treated with inattention. But within the last ten or twelve years, various observers both in England and abroad, have by their researches tended, directly and indirectly, to render knowledge on this subject more precise and more certain. Among these may be mentioned Dr Robert Lee, Dr Montgomery, Dr Martin Barry, Dr Allen Thomson, Dr Robert Paterson, M. Raciborski, and M. Deschamp. The essays of Dr Robert Paterson published in the fifty-third, fifty-fourth, fifty-fifth, and sixty-second volumes of this Journal, may be carefully studied for the important facts which they contain; and in the fifty-fifth volume (p. 474) is given an abstract of the elaborate researches of Dr Martin Barry. In the fifty-ninth, sixty-first, and sixty-third volumes, p. 492, accounts are given of the doctrines of M. Raciborski; and in volume sixty-third, at p. 236, is given a notice of the inferences of M. Deschamp; from which it may be seen what importance these physiologists attach to the influence of the ovaries over the uterine circulation and secretion.

Notwithstanding all the evidence furnished by these several inquirers, whether it be from the uncertainty and occasional contradiction of the facts, or from the natural obscurity of all inquiries relating to these organs, many persons entertain strong doubts of the correctness and justice of the doctrine of what has been termed the Ovarian theory or Ovular theory of Menstruation. If the cases given in the paper by Dr Letheby be not sufficient to dispel entirely these doubts, it must be allowed that they are well calculated to corroborate the theory, and render it at once more credible and more intelligible than it has hitherto been regarded.]

The observations that have been made at various times, during the last thirty years, by Messrs Power, Lee, Barry, Wharton Jones, Girdwood, and others in this country, together with the experimental researches of MM. Valentin, Gendrin, Wagner, Bischoff, Pouchet, and Raciborski on the Continent, have, I think, clearly proved that the phenomena manifested during the period of the catamenia in women, are closely connected with those observed during the time of heat or rut in quadrupeds; and that both of these phenomena are dependent on one cause, namely, the maturation of ovules. But while this hypothesis has been very generally admitted, there is, I believe, a tendency in the minds of many physiologists of the present day, to doubt whether the ovules so matured are ever extruded from the ovary and carried into the Fallopian tubes, without the stimulus of impregnation, or, at any rate, without the congress of the male. In support of this view, or rather of these doubts, an appeal is often made to the fact, that an ovule has never yet been detected in either of the Fallopian tubes of a virgin, who has died during the period of the catamenia, notwithstanding that many subjects have been examined, that most careful search has been instituted, and that appearances have frequently been noticed indicating the recent rupture of a Graafian follicle. In point of fact, it is imagined by those who entertain such doubts, that the fecundation of the germ takes place while it is within the Graafian follicle, and consequently, that if the ovule fails to be the subject of impregnation it never quits the ovary, but perishes within its formative vesicle. On the other hand, the researches of Bischoff have led him to enunciate a law, the purport of which is the very reverse of the preceding; for he says, that "the ovules formed in the ovaries of females of the human species and of mammiferous animals, undergo a periodical maturation, quite independently of the male seminal fluid. At these periods, known as those of heat or the rut in animals, and menstruation in the human female, the ovules which have become mature, disengage themselves from the ovary and are extruded. If the union of the sexes takes place, the ovule is fecundated by the direct action of the semen upon it. If no union of the sexes occurs, the ovule is nevertheless extruded from the ovary, and enters the Fallopian tube, but there perishes.*"

The law, as thus expressed, is in conformity with the opinions entertained by Drs Robert Lee, Paterson, Girdwood, Gendrin, Pouchet, Raciborski, Mr Wharton Jones, and many other authorities of the present time. It is also in accordance with the more ancient doctrines of Malpighi, Sir Everard Home, and Dr Power. Nevertheless, as the truth of this law, in its application to the human female, appears to be still open to the evidence of positive proof, I have thought it desirable to publish a report of the two following cases.

Case 1st.—November 20, 1850.—A woman, aged twenty-six, in a state of great mental excitement, attempted self-destruction by cutting her throat. The wound which she inflicted was not dangerous; and after having been attended to by a surgeon for a few days, the woman was removed to the London Hospital, where she became a patient under Mr Curling. She lingered until the 14th day of December, when she died. On the following day the body was examined, and it was noticed that the pelvic viscera were highly congested, that the uterus was considerably enlarged, that the vagina contained a sero-sanguineous fluid, and that the hymen was unruptured. In consequence of these appearances the parts were removed for further examination.

On cutting into the uterus, I discovered that it contained a small quantity of sanious fluid; I noticed, moreover, that both of the ovaries presented a number of stellate fissures or cicatrices on their surfaces; and that at one part of the left organ, namely, at its inferior, inner and posterior border, there was a distinct purple spot of the size of a small pea. In the centre of this spot there was a ragged opening, that had, in all probability, recently given exit to an unimpregnated germ. An incision was made into the gland, so as to cut through the discoloured portion of it, and it was then remarked that the aperture led into a small cavity, the existence of which is still evident in the wet preparation. The cavity was situated at the very summit of the spot, immediately within the opening on the peritoneal surface; and it was surrounded by a large quantity of what appeared to be extravasated blood. After the preparation had been immersed in spirit of wine for a few days, I was able to perceive that the spot was made up of four distinct parts;—1st, Of an outer vascular layer which surrounded the mass, and extended into it to the depth of $\frac{1}{16}$th of an inch; this layer was gradually thinned as it approached the aperture on the surface of the follicle; and it seemed to consist of *the stroma of the ovary* in a highly congested state. 2d, Of a thin layer of very dark matter, which appeared to be the remains of *the ovisac*. 3d, Of a mass of coagulated blood strengthened by a network of intersecting fibres; this mass had the bulk of a hempseed, and it was found to be composed of blood-discs, fibrin, and large granular corpuscles; it was, doubtless, therefore, the remains of *the tunica granulosa* infiltrated with blood. 4thly, and lastly, there was *the cavity* to which I have before alluded.

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* Beweis der von der Begattung unabhängigen periodischen Reifung und Loslösung der Eier, p. 4, quoted by Drs Baly and Kirkes in their Supplement to the second volume of Müller's Physiology, p. 45.

When the ovaries had become firm and hard by the coagulating action of the spirit, sections were made into them at various parts; by which means a number of yellow bodies (false *corpora lutea*) in different stages of degeneration were brought into view. Some of these bodies were rather large, and one of them contained a well-defined clot, the summit of which communicated with a cicatrix on the surface of the ovary. This circumstance led me to conclude that it was the remains of an old Graafian follicle, from which, at perhaps the last catamenial period, an ovule had escaped.

The Fallopian tubes were highly congested, especially at their fimbriated extremities, where, from the abundance of turgid capillary vessels, the tubes assumed a bright scarlet appearance. The cavities of the oviducts were filled with, and much distended by a thick bloody mucus, which readily escaped from their peritoneal apertures when the tubes were subjected to slight pressure between the finger and thumb. Both of the Fallopian tubes were carefully laid open by means of a pair of fine scissors—the operation being conducted on a clean white plate, containing a little water,—and their contents were minutely examined. The right tube did not present any object worthy of notice; but the left one contained, at about one inch from its distal extremity, a small white vesicular-looking body, which on being floated out into the water, was found to be rather ragged on its surface, and to have the size of the cavity noticed in the recently ruptured Graafian follicle. This body was submitted to microscopical examination. When viewed as an opaque object, nothing could be made out beyond the fact that it was covered with white flocculi. It was then placed between two pieces of glass, and examined by the aid of transmitted light; but it was too opaque for the eye to distinguish its structure, notwithstanding that the flocculi were very translucent and were seen to be made up of oval nucleated cells. By the employment of slight pressure the body was readily crushed, and then I could perceive that it was composed of a mass of nucleated cells among which at one part there was a number of highly refractive oil-globules. The result of this investigation led me to think that the body in question was an ovule, the elements of which had been so far disarranged by the pressure, that the *membrana granulosa* and *yelk-globules* were the only recognisable constituents of it.

The fluid contained in the uterus and Fallopian tubes were likewise subjected to microscopical examination. That removed from the former was found to consist of numerous blood-discs, most of which were strongly beaded at their edges; of much cylindrical epithelium, some of which was distinctly ciliated; of a large quantity of granular corpuscles, like exudation cells; of a few white globules, similar to those found in blood, many of which had apparently passed into the form of spindle-shaped or fusiform bodies by the elongation of their opposite ends; and of a thick gelatinous fluid which united all the elements together. That from the latter, namely, the Fallopian tubes, was much the same as the preceding, excepting that the number of the blood-discs was considerably less; that there was a greater abundance of ciliated epithelium; and that the fluid in which the elements floated was not gelatinous, but serous.

A consideration of the facts thus presented to notice, led me to conclude that the girl had died at the very onset of a catamenial period, for I could not discover any evidence of the occurrence of an external flow; in fact, the secretion found in the vagina was not very abundant, and it had acquired only a pale rose tint.

On instituting a further inquiry into the case, I ascertained that the periodical flux had taken place exactly one week before the woman made the attempt on her life; and with regard to the subsequent history of the case, it may be said that she was laid up with the wound in her throat for a period of twenty-four days before her death, nineteen of which were passed in a separate ward of the Hospital, where, in consequence of her very distressing condition, she was closely watched by a female attendant, so that it is hardly possible that sexual intercourse could have been effected during that period of time.

While I was engaged in the investigation of the preceding case, I received from my friend Dr Parker of Finsbury Square, who had assisted me in the foregoing inquiry, another uterus and its appendages, which he had removed from the body of a lunatic aged twenty-three. This girl had died, and was examined in St Luke's Hospital, where she had been a patient for eleven months, under circumstances which deprived her of the opportunity of associating with a male for a long period before her death.

The information obtained by inquiries of the attendant at St Luke's, as well as by an examination of the organs themselves, led me to conclude that the girl had quitted life during the catamenial period; for the pelvic viscera were much congested, the uterus was considerably enlarged, its vessels were turgid, and its cavity contained a red jelly-like matter; besides which, the Fallopian tubes were filled with a thick muco-sanguineous secretion, and the right ovary presented a dark livid spot on its outer and lower part: many cicatrices were also found on the surfaces of both the ovaries. As in the last case, the livid spot had a hole in its centre; and, on making a section of the ovary so as to divide it through the spot and an adjacent cicatrix, I perceived that the hole led into a cavity, which was surrounded by a deep red tissue, and that the cicatrix communicated with a very perfectly formed *corpus luteum,* having a central cavity containing a dark red clot.

The matter contained in the right Fallopian tube was submitted to careful examination, by which means I discovered a little globular body that had the size of a small pin's head. This body was transferred, as in the last case, to water; then placed between two pieces of glass, and examined under the microscope with a power of 100. The outer constituents of the mass were precisely like those of the preceding; that is, they consisted of nucleated cells, arranged so as to form a shaggy, but tolerably compact tunic. At one end of the object, near to its surface, there was a transparent ring, inclosing a rather opaque granular mass, in which there was an eccentric highly pellucid spot. I had no doubt that this was the ovule, consisting of the *zona pellucida*, the *yelk*, and the *germinal vesicle;* but, to test the truth of my opinion, I subjected the mass to the action of a little strong acetic acid, by which means the *zona pellucida* was still more clearly brought into view; for it happened that the nucleated cells of the *membrana granulosa* were slightly corrugated by the acid, and, as it were, contracted on their contents. They also acquired a greater transparency; and by using a power of 300, the *zona* presented a distinctly striated appearance, and the granules within it were seen to be highly refractive. Lastly, the whole object was washed with ether, which dissolved away the fat granules, and thus left the *zona* and *vesicle* still more distinct. In the place of the yelk there was now left a somewhat opaque structureless material, which seemed to have been the bond of union between the numerous fatty elements of the vitelline mass.

The fluid matters contained in the uterus and Fallopian tubes, were identical in their physical characters with those noticed in the last case; and the materials composing the recently ruptured Graafian follicle were likewise found to consist of blood-discs, fibrin, and large nucleated cells. The yellow tissue of the *corpus luteum* was made up of a fibro-cellular stroma, in which there was inclosed a number of large granular corpuscles, some of which contained oil-globules; besides these, there was a liquid fat diffused through the tissue, which appeared either as minute particles, or as large elongated highly refractive globules that had been produced by the union of the smaller elements.

Remarks.—An examination of the preceding facts will, I think, justify me in coming to the following conclusions; namely, *that ovules escape from the ovaries of women during the catamenial flux; and that the escape of these bodies is spontaneous*, id est, *that their extrusion takes place independently of sexual intercourse.* In these respects, the phenomena witnessed are perfectly analogous with those observed by Bischoff, Raciborski, Pouchet, and others, while they were instituting inquiries in the changes which occur in the ovaries of other mammals during the periods of heat and rut; and they are also in accordance with a fact long since ascertained by physiologists, namely, that there is a periodical maturation and spontaneous extrusion of ovules from the ovaries of animals still lower in the scale of creation. Hitherto, however, these several circumstances have been regarded only in the light of probable analogies. But I am of opinion that the facts detailed in this paper are quite sufficient to identify the phenomena in question, and also to establish the truth of a great part of the law enunciated by Bischoff, that is, that the ovules formed in the ovaries of females of the human species, and of mammiferous animals, undergo a periodical maturation, quite independently of the influence of the male seminal fluid; and that at the periods of menstruation in the one, and heat in the other, the ova which have become mature disengage themselves from the ovary, and are extruded whether there be access of the male or no. But admitting that this law is in great part the expression of truth, we have yet to determine whether in human females, as in the females of other animals, the maturation and escape of ovules take place only at the fixed periods mentioned, or whether the discharge of these bodies is always occurring, thus rendering the human female susceptible of impregnation at all times.

The final solution of these questions, together with that of the problem concerning the length of time that the ovule retains its faculty of being fecundated after it has quitted the ovary, is of great interest to the physiologist, the obstetric physician, and the medical jurist; for it would not only enable them to pronounce with certainty at what time conception usually takes place, but it would also furnish a starting-point for the determination of other equally important problems, viz., the ordinary, and the most extended periods of human gestation. It must, however, be evident that a great number of independent observations have yet to be made, before any hypothesis relating to this part of the subject can take its place amongst the well-recognised doctrines of science; and, believing that each observation must have some weight and value, I have been led to record the results of my own inquiries, in order that they may be placed in juxtaposition with the facts already elicited by more able investigators.

Here, perhaps, I may be allowed to remark, that in addition to the experiments and observations so admirably reviewed by Drs Baly and Kirkes in their Supplement to the second volume of Professor Muller's Physiology, two cases have been published, in which the phenomena witnessed are in many respects very similar to those noticed by myself. In one of these cases, the girl died shortly after menstruation; and the reporter, M. Janzer, states that, in making an examination of her body, he found on the surface of the left ovary a dark red spot, which had a fissure in its centre. Judging from all the circumstances of the case, Janzer was led to conclude that the spot in question was a recently ruptured Graafian follicle gorged with blood. He instituted a dilligent search for the liberated ovule, but he failed to discover it*.

The second case is recorded by M. Locatelli;† and he informs us that the woman who was the subject of his investigations died from the effects of an operation made for the relief of an imperforate hymen. He noticed that on one of the ovaries there was a livid spot, in the centre of which there was an aperture. The parts were carefully examined, and the author believes that the spot seen in the ovary was a recently ruptured follicle filled with blood. As in the last case, however, the liberated ovule escaped detection.

The facts communicated in the foregoing memoir are illustrated by several correct lithographic drawings. But for these the reader is referred to the Philosophical Transactions.

ART. III.—*Researches into the Structure of the Spinal Cord.*‡ By J. LOCKHART CLARKE, Esq. Communicated by SAMUEL SOLLY, Esq., F.R.S. (From the Philosophical Transactions of the Royal Society for 1851. Part II., p. 607.)

Received Oct. 15.—Read Dec. 5, 1850.

By reflecting on certain facts connected with the respiratory movements, and which seem to derive no explanation from our actual knowledge of the structure of the spinal cord, I was induced to undertake a series of observations, with the view of determining, if possible, the relations which appear to subsist between the spinal nerves and the respiratory nervous centres. These observations, however, led me into a more extended inquiry than I at first contemplated, the results of which I now venture to lay before the Royal Society.

It is needless to point out the difficulties which attend, not only the minute investigation, but a clear and connected description, of a structure so intricate and delicate as that of the spinal cord. It may be proper however, to state, with regard to the contents of this paper, that those facts only which were verified by cautious and repeated examination have been brought forward with confidence; while in cases where the results of my observations were less satisfactory, I have expressed myself with corresponding reserve. Yet no labour has been spared, in order to arrive at correct conclusions. My observations were made by means of one of Mr Ross's finest microscopes, on many thousand preparations of the spinal cord of Man, of the Calf, Sheep, Pig, Dog, Cat, Rabbit, Guinea-Pig, and Frog, and occupied a period of more than two years.

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* Heidelberger Annalen, Bd. xiii. p. 601–604.

† Frorieps Neue Notizen, Bd. vii. p. 348–350.

‡ At the time these inquiries were begun, and nearly up to the period of their completion, I had not seen any of the works of Dr STILLING, and knew no more of his investigations into the structure of the spinal cord than what I had gathered from the anatomical and physiological works published in this country. However nearly, therefore, a few of the facts brought forward in this paper may correspond to the results of his inquiries, I may state that they were made out without any knowledge of Dr STILLING's views, excepting only those which regard the origin of the spinal nerves. On shewing some of my preparations to Mr SOLLY and Mr GRAINGER, I was advised by those gentlemen to look over the works of Dr STILLING; and having done so, I shall refer to them whenever they appear to oppose, or coincide with, my own observation.

In making these preparations, the two following methods were adopted:—a perfectly fresh cord was hardened in spirits of wine, so that extremely thin sections, in various directions, could be made by means of a very sharp knife. A section so made was placed on a glass slide and treated with a mixture composed of one part of acetic acid and three of spirits of wine, which not only makes the nerves and fibrous portion more distinct and conspicuous, but renders also the grey substance much more transparent. The section was then covered with thin glass, and viewed first by reflected light with low magnifying powers, and then by transmitted light with higher ones.

According to the second method, the section is first macerated for an hour or two in the mixture of acetic acid and spirit. It is then removed into pure spirit, and allowed to remain there for about the same space of time. From the spirit it is transferred to oil of turpentine, which expels the spirit in the form of opaque globules, and shortly (sometimes immediately) renders the section perfectly transparent. The preparation is then put up in Canada balsam and covered with thin glass. By this means the nerve-fibrils and vesicles become so beautifully distinct, that they may be clearly seen with the highest powers of the microscope. If the section be removed from the turpentine when it is only semi-transparent, we sometimes obtain a good view of the arrangement of the blood-vessels.*

All the observations described in this paper were made and verified also on sections in a perfectly fresh state, and unaffected by any chemical re-agent; and whenever it was proposed to examine the natural structure of any particular part, as the nerves and vesicles, a specimen was selected from an animal immediately after death.

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* This mode of preparation succeeds best in cold weather; for in summer, the cord, however fresh when immersed in the spirit, remains more or less spongy, instead of becoming firm and dense in the course of five or six days. The spirit should be diluted with an equal quantity of water during the first day, after which it should be used pure. Certain modifications of this mode of preparation may be sometimes employed with advantage by a practised hand.

The drawings have been executed by myself with the greatest care, and may be relied on as faithful and exact delineations of what was seen under the microscope. The outlines of all were taken by means of a camera lucida, in order to ensure correctness.

Of the Grey Substance of the Spinal Cord.

At the lower end of the spinal cord, the posterior grey substance consists only of a single mass, having a broad and rounded extremity, and formed, as we shall presently see, by the coalescence of the two posterior cornua along their inner borders, where, being increased in breadth, they meet each other in the middle line. In consequence of this arrangement, the *substantia gelatinosa* extends uninterruptedly and horizontally across from one side to the other. From the posterior white columns it is separated, particularly on each side, by a kind of border composed of fine nerve-tubes which wind round it and proceed from the posterior roots of the nerves.

The anterior grey substance, on the other hand, is here, as in other regions of the cord, separated by the anterior white columns into two cornua of considerable size. These curve somewhat inwards, taper to an irregularly rounded point, and nearly reach the circumference of the cord.

Here also the spinal canal is very distinct and large, being the $\frac{1}{100}$th part of an inch in diameter. It is situated much nearer the anterior than the posterior surface of the cord.*

The nerve-vesicles however are few in number, and scattered irregularly through the grey substance. They are not, as in some other regions, collected into large masses, but are most numerous in the anterior cornua, at the extremities of which three or four may be seen closely grouped together.

Nature and Arrangement of the Nerve-fibres in the Grey Substance of the Spinal Cord.

The nerve-fibres in the grey substance of the spinal cord are mostly of the white tubular kind, and of variable but small average diameter.† With a regard to *direction*, they may be divided into two clases,—*transverse* and *longitudinal.*

Transverse Fibres.—These again may be divided into two orders,—*antero-posterior* and *latero-transverse.*

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* As stated by Stilling and Wallach, it extends uninterruptedly through the whole length of the *medulla spinalis.* In many preparations that I have by me of the filiform extremity of the cord, where it measures less than a line in diameter, the spinal canal is even larger than in other regions.

† By Drs Stilling and Wallach they are described as grey fibres, but seem to have been examined under strong pressure. Moreover, they ought to be examined in a perfectly fresh state, as spirits of wine considerably alters their appearance.

Antero-posterior Fibres.—On each side of the middle line, in the posterior grey substance, are several large bundles of fine nerve-tubes, which take a transverse direction from behind forwards. These bundles may be traced through the posterior white columns, partly into the roots of nerves which arise in the same vertical (in Man, horizontal) plane, and partly into others above and below them. They are convex towards the middle line, but on approaching the anterior cornua, they break up into smaller bundles, which interlace with each other and form a coarse network, in the meshes of which the nerve-vesicles are contained. Many of these nerve-tubes, both singly and in bundles, extend outwards into the antero-lateral white columns.* Many of them, also, appear to be continuous with the anterior roots of the nerves; but whether they be so or not, the anterior and posterior roots are mingled together in the network above mentioned.† Between the spinal canal and the anterior median fissure, some of these fibres cross obliquely through the anterior white columns, and decussate with corresponding fibres from the opposite side.

Latero-transverse Fibres.—The second order of transverse fibres take a more or less horizontal direction between the opposite sides of the grey substance, forming the transverse commissure, and are arranged chiefly in loose bundles. Those behind the spinal canal diverge on each side into the anterior and posterior grey masses, and extend into both the posterior and lateral white columns; a great number being evidently continuous with both the anterior and posterior roots of the nerves. One large bundle in the centre is particularly conspicuous. Those in front of the spinal canal are much less numerous, and curve principally forwards; they may be traced partly into the antero-lateral columns, and partly into the anterior roots of the nerves.

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* After nearly reaching the circumference of the cord, they *appear* to form with each other a series of loops of various sizes; but of this arrangement I cannot speak with any confidence, for it is seen only in a section of a perfectly fresh cord, and under strong pressure, to which the appearance may be due. STILLING (Textur des Rückenmarks, p. 21) also states that the transverse (so-called) *grey* fibres form loops at the periphery of the cord, but since he made use of strong compression, his statement is open to the same objection.

† This description corresponds, in some measure, with that given by Dr STILLING (Ueber die Medulla Oblongata, p. 4), who states that the posterior nerves, after forming loops within the grey substance, become continuous with the anterior roots. I have never seen loops such as he describes; but the rest of his statement is probably correct; for although it is impossible to trace any *single* fibre from the posterior to the anterior roots, it is nevertheless certain that the latter are continuous in the grey substance with *bundles* which proceed from the former. According to STILLING, therefore, the anterior and posterior roots of the spinal nerves are united in the grey substance by intervening *grey* fibres, since all the transverse fibres are described as such; or to use his own words "Die Primitivfasern der Nervenwurzeln nichts Anderes sind als die unmittelbaren Fortsatze der queerlandfenden grauen Substanz des Rückenmarks" (Textur des Rückenmarks, p. 28). The transverse fibres, however, are really white tubules of smaller diameter than those which form the roots of the nerves.

By Dr STILLING, some of the fibres of the posterior nerves are said to cross over, behind the spinal canal, into the anterior cornua of the opposite side. I have many times thought that such an arrangement really exists; but the inquiry is attended with great difficulty; and my own observations, though frequently repeated, are too uncertain in their results to admit of my confirming or denying the existence of these fibres. HANOVER denies that there is any crossing of the fibres in the spinal cord. He says, "Une partie des fibres passe bien d'un côté de la moëlle épinière à l'autre, mais un entre-croisement géneral n'a pas lieu."*

Longitudinal Fibres.—The fibres of the *substantia gelatinosa* are, as usually described, chiefly longitudinal. When examined immediately after death, and uninjured by pressure or traction, they have all the characters and appearance of minute tubular fibres, presenting dark, single, and sharply-defined outlines, with frequent varicosities; but when stretched, or otherwise injured by manipulation, they lose their dark borders, and become exceedingly pale and delicate.† They vary but little in size, but are all extremely slender, their average diameter not exceeding the $\frac{1}{10\cdot000}$th of an inch; some few are larger, but I have seen numbers as small as the $\frac{1}{15,000}$th or $\frac{1}{16,000}$th of an inch in diameter, as shewn by the micrometer. They are usually distinguished as grey fibres, but differ in many respects from the grey fibres contained in the sympathetic system of nerves; for they are not only, on an average, finer, but, like the tubular fibres, are little or not at all affected by acetic acid, which fails to bring out any traces of nuclei.‡ They have also been said to resemble the

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[*] Recherches Microscopiques sur le Système Nerveux, p. 14.

[†] The method adopted for examining these fibres was as follows:—a thin longitudinal section through the *substantia gelatinosa* was made by a rapid stroke with a very sharp instrument wet with albumen. The *substantia gelatinosa* was then carefully separated from the parts on either side of it, and examined without any pressure beyond that produced by the weight of the thin glass employed to cover it.

[‡] REMAK states that they are nucleated. Drs STILLING and WALLACH describe them as grey nerve-tubes presenting a very small diameter and a bright golden colour; but add, that they are not easily examined with accuracy. Repeated and careful examination, however, has convinced me that the above description is correct; their golden colour, when seen by transmitted light, is due only to the granular matrix in which they lie; when viewed by direct light, they have an opaque white appearance.

processes of the caudate vesicles, but in their natural state they certainly bear no resemblance to them whatever. Around the extremity of the spongy portion of the posterior cornua, these longitudinal fibres are collected into an arched and very dense band, of the same form, and about the same thickness as the *substantia gelatinosa*, with the lower border of which it is continuous. This band may be seen even with the naked eye, and then appears as a white opaque stripe. The longitudinal fibres are also found, but in smaller numbers, through the rest of the grey substance.

Changes in the Form of the Grey Substance.

On examining the cord upwards towards the lumbar enlargement, certain modifications are found to take place in the form and disposition of the grey masses. The two halves of the posterior mass, with its two sets of antero-posterior bundles of tubular nerve-fibres, begin to separate from each other at the middle line. Here the *substantia gelatinosa* is first interrupted, and the posterior border of the grey substance between its divided central extremities, or rather, the posterior fibres of the transverse commissure, begin to arch forwards towards the spinal canal, and to mark out the rudiments of the posterior cornua. At the same time, on each side, and a little behind, the spinal canal, a small but gradually increasing mass of caudate vesicles makes its appearance. These are the commencement of two longitudinal columns of vesicular substance, which extend through the whole length of the spinal cord, and which I name the *posterior vesicular columns*. They are traversed and surrounded by fibres from the posterior roots of the nerves, and also by arched fibres of the posterior transverse commissure, some of which, as already stated, are continuous with the anterior roots of the nerves. The changes just described continue to increase from below upwards, and reach their greatest extent in the middle of the lumbar enlargement, where the posterior cornua are broad and long, and widely separated from each other by the posterior white columns of the cord; while the space between the posterior border of the transverse commissure and the spinal canal, and which is almost entirely occupied by the former structure, is much reduced in breadth, for it measures only the $\frac{1}{230}$th of an inch, whereas at the lower extremity of the cord its diameter reaches the $\frac{1}{60}$th of an inch. At this central constricted portion, therefore, of the transverse commissure, its fibres, which before were but loosely arranged, are now compressed into a dense band, but still diverge on each side into the anterior and posterior cornua. In consequence of these changes, also, the two *posterior vesicular columns*, which have increased considerably in size, and were formerly situated behind, and at the sides of, the spinal canal, are now pushed up, as it were, and included in the posterior cornua, some of their vesicles reaching as high as the *substantia gelatinosa.* As a further result of the same changes, the posterior white columns are much deeper and broader here than in the lower regions of the cord.

While the posterior masses of the grey substance are undergoing the modifications just described, a series of somewhat similar alterations are found to take place in the form and arrangement of the anterior masses. In the middle of the lumbar enlargement, the anterior cornua have increased to a still greater extent than the posterior, and have assumed a shape the opposite of that which they possessed lower down in the cord. They now turn rather outwards instead of inwards, and have a large irregularly club-shaped, instead of a pointed, extremity. The caudate vesicles have become exceedingly numerous, and are grouped together in several large masses, which are situated chiefly on the outer and middle parts of the cornua. They lie in the meshes of a network formed by bundles of fibres proceeding from the anterior and posterior roots of the nerves, and from the commissural bands behind the spinal canal.*

On proceeding upwards from the lumbar enlargement into the dorsal region of the cord, arrangements in the grey substance are observed to take place in the reverse order of those already described. By degrees the posterior cornua are reduced in length, and somewhat modified in shape; their inner sides extend towards each other and the middle line, while the posterior bands of the transverse commissure are drawn, as it were, gradually backwards, becoming at the same time less curved; so that the space between them and the spinal canal is now correspondingly increased. Into this space the two *posterior vesicular columns* advance between the fibres of the transverse commissure, together with the inner sides of the posterior cornua, which, in the middle of the dorsal region, coalesce and inclose them behind. Here, then, the posterior grey substance again consists only of a single mass, and again, also, the *substantia gelatinosa* extends uninterruptedly and nearly horizontally across from side to side. On the other hand, the anterior cornua are long, straight, and narrow, and project almost directly forwards. The caudate vesicles, reduced considerably in number, are scattered irregularly through them, but are more numerous towards their extremities, where they are sometimes seen in the form of one or two small groups.

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* It is interesting, in a physiological point of view, to find that the number of caudate vesicles is in direct proportion to the size of the nerves, which are known to be much larger at the lumbar and cervical enlargements than in other regions.

From the middle of the dorsal region to the cervical enlargement of the cord, the alterations in the form of the grey substance are once more reversed, being nearly similar to those which are found to take place on proceeding upwards from its lower extremity. The posterior mass renews the process of division into two parts, commencing with the *substantia gelatinosa* at the middle line, where the transverse commissural bands are gradually pressed, as it were, forwards, and thus mark out the posterior cornua; while in these, which again reach their greatest size in the middle of the cervical enlargement, the *posterior vesicular* columns, formerly situated behind and at the sides of the spinal canal, at length resume their position and increased dimensions. During the progress of these changes, the anterior cornua also undergo considerable modifications. They become gradually broader and longer, and contain a much larger number of vesicles, which in the cervical enlargement are grouped together, as in the lumbar region, into several large masses; indeed, the general arrangements of the grey substance at these two parts of the cord have a very striking resemblance.*

At the outer border of the grey substance, between the anterior and posterior cornua, is a small column of vesicular matter, which is softer and more transparent than the rest. This consists of caudate vesicles of inferior size and more regular shape than those usually found in the spinal cord. It is seen distinctly at the upper part of the lumbar enlargement, and increases somewhat in size in the dorsal region, where it projects slightly into the lateral column on each side. In the cervical enlargement it is less distinctly marked, but higher up it again becomes conspicuous, and is there seen to form the principal part of the nucleus of the spinal-accessory nerve. On ascending the *medulla oblongata*, this column of vesicles gradually makes its way inwards till it reaches the space immediately behind the spinal canal, where it meets and blends with its fellow of the opposite.†

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* The vesicles are more abundant, however, in the lumbar than in the cervical enlargement, and therefore correspond in number to the relative size of the nerves which belong respectively to these regions.

† The nucleus of the spinal-accessory nerve in the *medulla oblongata* has been already correctly described by Dr STILLING, Die Medul. Oblong.

In the upper part of the cervical region the posterior cornua are long and narrow. The outer part of each, below the *substantia gelatinosa*, consists of a large and remarkably beautiful network of bloodvessels, which incloses in its meshes bundles of fibres of the lateral columns. These bundles are of various sizes, and encroach gradually upon the cornua as they ascend to the *medulla oblongata*. Some of the fibres of the spinal-accessory nerve run transversely through this network; but a considerable branch, on entering the grey substance, bends *forwards*, and after passing through its vesicular nucleus, continues the same course *within the anterior cornua, where its fibres traverse and surround the caudate vesicles, in company with the roots of the anterior spinal nerves.* The space behind the spinal canal has slightly increased in breadth, and the *posterior vesicular columns* on each side of it have nearly the same position and relations as in the dorsal region. The anterior cornua are rather small and pointed, and contain each a circular or oval mass of vesicles.

The central portion of the grey substance surrounding the spinal canal is described by FOVILLE* as the grey commissure of the cord; and by STILLING† as a circular commissure composed of exceedingly delicate grey nerve-fibres. To me, however, its structure appears to consist of a circular layer of extremely fine fibrous tissue (for supporting the sides of the canal), left unobscured by the transverse commissural fibres which arch round it in front and behind. Indeed it may be seen at some parts of the cord to be continuous with the areolar tissue, which extends from the borders of the anterior fissure through the white columns in front of the canal. It varies also in shape at different parts of the cord, being fusiform from side to side in the higher regions, where the curves described by the anterior and posterior transverse commissures are least; and nearly circular in the lower regions, where those curves are greatest. The walls of the spinal canal are lined with a layer of columnar epithelium, which, in a transverse section of the cord, viewed by direct light, appears as a white opaque ring at the margin of the foramen.‡

Of the Nerve-vesicles of the Spinal Cord.

The vesicles found in the grey substance of the spinal cord are either circular, oval, pyriform, or otherwise irregular in shape; and all of them, except those peculiar to the *substantia gelatinosa*, have remarkably delicate processes issuing from their sides, like the neck from a flask or funnel. They are found chiefly in the anterior cornua and in the *posterior vesicular columns*.* They are also connected with each other by their processes, which divide and subdivide into smaller branches, so that the space between them appears to be occupied by a minute network of the most delicate fibrils.† Many of these processes, particularly from vesicles situated near the border of the grey substance, run out into the white columns through fissures which contain bloodvessels and *pia mater*. Whether they give off branches which follow the vascular network through the white columns, I have not been able to determine. That the caudate vesicles have some important relation to the functions of the nerves, there is every reason to believe, since we find that they not only *invariably exist in the vicinity of nerves*, but, as already shewn, that *they increase also in number in direct proportion to the size of the nerves with which they are associated*. I am aware that several continental physiologists of eminence assert that they have discovered in the lower vertebrata and in the invertebrata a direct union between the caudate vesicles and the tubular fibres; but I have never been able to make out satisfactorily any such connection in the spinal cord of mammalia, although I have constantly sought for it under very favourable circumstances.‡ The nerve-fibrils wind around, and apparently in contact with, the vesicles, but the connection does not seem to have at all the character of an attachment. Nor have I succeeded in tracing anything like continuity between the tubular fibres and the *processes* of the vesicles. It is very common to see one or two of these processes running outwards into a bundle of nerves attached either to the anterior or posterior cornua; but then we have seen that they pass out into the white columns from *all* sides, and therefore from parts which are not connected with nerves.

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* Traité Complet de l'Anatomie, &c. du Système nerveux Cerebro-spinal.

† Textur des Rückenmarks, p. 23.

‡ It was suggested to me by Mr BOWMAN of King's College, that as the spinal canal is continuous with the fourth ventricle, it is probably lined with epithelium, which, on careful examination, I found to be the case.

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* The situation and connections of these two longitudinal columns of vesicular substance render them extremely interesting. They would seem to have some intimate relation to the functions of the posterior roots of the nerves, many of which, as already stated, traverse and surround them, without however forming with their vesicles any apparent connection. At the upper part of the *medulla oblongata* they are reduced in size and ultimately disappear. The processes radiate from their vesicles on every side; some extending to the extremities of the posterior cornua, and others into the lateral and posterior white columns.

† A somewhat similar description is given by TODD and BOWMAN, Physiological Anatomy, p. 214.

‡ HANOVER is, I believe, the only observer that professes to have seen it in all classes of vertebrata.

Bloodvessels of the Spinal Cord.—These enter through the anterior and posterior median fissures, through the smaller fissures in the white columns, and at the roots of the nerves. After giving off numerous branches to the white columns, they proceed inwards to the grey substance, along the whole periphery of which they form a remarkably beautiful network of loops,* intermixed with nerve-tubes and some of the processes of the vesicles. Within the grey substance they form an extremely minute network. Some of the vessels which enter at the anterior and posterior fissures and at the posterior white columns, anastomose with each other around the spinal canal; while others branch off, right and left, between the bundles of the transverse commissure, into the anterior and posterior cornua, where they follow the course of the transverse fibres, and running to the periphery of the grey substance, assist in forming there the loops already described. A series of loops is also found to exist near the extremity of the posterior cornua, along the border of the spongy substance.

Of the White Columns of the Spinal Cord.

The anterior white columns of the cord have no proper transverse commissure, but are united in the middle line, at the bottom of the fissure, by a fibrous band or raphè. In this situation, however, they are crossed transversely by horizontal and oblique tubular nerve-fibres and bloodvessels proceeding from the grey substance on either side, and which, in some regions of the cord, are so numerous that they nearly replace the longitudinal fibres.† The transverse slits observed by **Foville** on each side of the raphè are fissures for the passage of bloodvessels.

Nor are the posterior columns of the cord connected by a transverse commissure; for the posterior fissure, as stated, first, I believe, by **Bellingeri**,‡ reaches down to the border of the grey substance, or rather to the posterior border of its transverse commissure, which has been mistaken and described by **Foville** as a commissure between the posterior white columns, formed by their coalescence at the bottom of the fissure.*

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* According to Dr **Stilling** this network consists of the (so-called) grey transverse fibres. He confesses, however, that it is not unlike a network formed of small bloodvessels.—(*Textur des Rückenmarks*, p. 22.)

† **Stilling** maintains that the separation of the anterior white columns is complete, for at the bottom of the fissure may be seen "the transverse grey fibres of the anterior commissure."—(*Ueber die Medul. Oblong.* p. 6.) The fibres, however, which give to this structure its grey or yellowish-grey appearance, are really bloodvessels and pia mater.

‡ De Medullâ Spinali, pp. 7 and 8.

Of the Origin of the Spinal Nerves.

Posterior Roots.—The bundles of fibres which form the posterior roots of the spinal nerves are much larger but less numerous than those of the anterior: the fibres themselves, however, are mostly finer and more delicate.† They are attached immediately to the *posterior* white columns *only*,‡ which they traverse obliquely inwards, interlacing and forming with each other an intricate plexus. From this plexus straight and distinct bundles§ enter the posterior cornua along their whole breadth, and cross the *substantia gelatinosa* both obliquely and at right angles; some being immediately continuous with fibres of the transverse commissure, while others, after plunging into the spongy portion of the cornua to unequal depths, break up and form a finer net-work which extends forwards into the anterior cornua; some of their fibres, after traversing the grey substance, pass out, as already described, into the posterior and lateral white columns.

Anterior Roots.—These are attached exclusively to the anterior parts of the antero-lateral columns. They do not always enter the cord directly, or at the points of attachment, but sometimes run for a short distance transversely along its circumference. They then traverse the antero-lateral columns somewhat obliquely, and in straight and distinct bundles, which do not, like those of the posterior nerves, cross and interlace each other, but proceed directly to the anterior grey cornu.* Their connection with the latter is still doubted by many physiologists of eminence, but in the preparations which illustrate this paper, it may be seen as clearly and satisfactorily under the microscope, as their connection with the white columns is perceived by the naked eye.† On reaching the grey cornua, they break up into smaller bundles and separate fibrils, which diverge in various directions. Some of the fibres proceed to the right and left, crossed by others from neighbouring bundles. Of those proceeding within the external border of the cornu, a few pass out to the antero-lateral columns; while others, after winding round groups of caudate vesicles, curve inwards and join the fibres of the transverse commissure. Of those proceeding along the inner border of the cornu, a few pass into the white column at the side of the median fissure; while others, on reaching its base, curve round, and, in company with fibres from the posterior cornu, cross obliquely through the junction of the anterior columns in front of the spinal canal, where they decussate with corresponding fibres from the opposite side. The remaining bundles of the roots plunge into the central portion of the cornu, and winding among its vesicles, are lost in the intricate network. A few of their fibres, after proceeding to some depth in the grey substance, bend round and take a more or less longitudinal direction.

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* He observes, "Elle semble plutôt la simple coalescence des deux faces latérales, réunies au fond du sillon." (*Op. cit.* p. 134.)

† I find that this statement coincides with that previously made by **Remak** (**Müller**'s *Archiv*, 1836), and by **Henle**, *Allg. Anat.*, p. 669.

‡ I believe that this statement is directly opposed to the opinions of almost every anatomist, except Sir **Charles Bell**, that has written on the subject of the spinal cord; and were it not undeniably proved to be true by the preparations which accompany this paper, I should feel some hesitation in coming forward to oppose so many eminent and recent authorities. According to **Bellingeri** (*op. cit.*) the posterior roots are attached to both the posterior and lateral, and the anterior roots to the anterior and lateral, white columns. Mr **Grainger** and Mr **Swan** join in referring the origin of both to the lateral columns only: the former observes (*Spinal Cord*, p. 80), "I have never been able to trace any fibres from the nerves into the fasciculi composing the anterior and posterior columns." A nearly similar statement is made by Dr **Todd**. The two other most recent writers on the subject (Drs **Sharpey** and **Carpenter**) have adopted the same views as **Bellingeri**. By Sir C. **Bell** (whose conclusion, however, was probably drawn from superficial examination), these roots are referred to the posterior columns only. "Each nerve," says he, "has two distinct series of roots coming out in packets or fasces, one from the posterior column, and one from the anterior column, of the spinal marrow." —(*Nervous System*, p. 29, 4to.)

§ The primitive fibres of most of these bundles are very small, their average diameter being about the $\frac{1}{7000}$th of an inch; a few very large tubules are found amongst the rest, and here and there is seen a slender bundle composed of fibres of larger average size. The anterior roots of the nerves within the grey substance are composed of fibres of more irregular size, and contain a much greater number of large tubes than those of the posterior roots.

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* I have not yet ascertained whether any of the fibres of the spinal nerves ascend with the white columns without entering the grey substance, as maintained by Mr Grainger, Mr Solly, and Dr J. Budge.

† Dr Sharpey, one of the most recent authorities on the subject, observes, "The anterior roots have been said to reach the anterior grey cornu, by passing through the superficial stratum of white substance over it, *but actual demonstration of the fact is yet wanted*."—(Quain's *Anat.* 5th edit., p. 727.) Bellingeri states merely that some of the anterior roots *perhaps* reach as far as the grey substance. His account of their origin is as follows: "Cernuntur filimenta radicum anteriorum nervorum spinalium partim exoriri à fasciculis anterioribus medullæ spinalis, et quidem filimenta aliqua nervea directè secedunt à superficie medullæ spinalis; alia verò filimenta nervea per canaliculum a piâ matre suppeditatum profundè descendunt in substantiam albidam medullæ, et *fortassè* nonnulla usque ad cineream substantiam perveniunt."—(*Op. cit.* p. 50.) It is well known that Mr Grainger was the first who professed to have traced the connection of the anterior roots of the nerves with the grey substance of the cord; and that Dr Stilling has since described and represented it; but the statements of these eminent men failed to convince most of the physiologists of this country. I may take this opportunity, however, of observing, that we are much indebted to Dr Stilling for having thrown considerable light on some of the most obscure points in the anatomy of the spinal cord and medulla oblongata; and for having originated and suggested new methods of further inquiry. His researches prove him to be not only a skilful, ingenious, and laborious investigator, but, in general, a correct and acute observer. The plates attached to his second work, although a little embellished, are, with a few trifling exceptions, exceedingly correct.

Of the Nerve-Tubes of the Spinal Cord.

The nerve-tube is endowed with considerable elasticity, and may be drawn out and attenuated to a great extent. It then loses its thick and sharply-defined borders, or double outline, and assumes the appearance of a much finer and more delicate fibre. When partially broken by traction, the two extremities are connected often by one of its borders only, and sometimes only by the axis cylinder, which has then the appearance of an almost imperceptible shadowy line; in the unbroken portion of the fibre, however, the axis cylinder appears perfectly pellucid.

The following summary of the principal facts described in this paper may perhaps be found useful.

That the posterior grey substance at the lower extremity, and in the dorsal region of the spinal cord, consists only of a single mass; and that the *substantia gelatinosa* there extends uninterruptedly across from one side to the other.

That the nerve-fibres of the grey substance, including those of the *substantia gelatinosa*, are not grey fibres bearing nuclei like those of the sympathetic, but fine tubules.

That two considerable columns of caudate vesicles (which I have named the *posterior vesicular columns*) in intimate connection with the posterior roots of the nerves, extend through the whole length of the cord, commencing small at its lower extremity, increasing in size in the lumbar and cervical enlargements, and terminating at the upper part of the medulla oblongata.

That the number of caudate vesicles, particularly in the anterior grey substance, is in direct proportion to the size of the nerves.

That the column of vesicles into which, in the cervical region, the spinal-accessory nerve may be traced, extends down the cord as far as the lumbar enlargement.

That a considerable branch of the spinal-accessory nerve, after entering the grey substance through the lateral column, may be easily traced to the caudate vesicles of the anterior cornu.

That the spinal-accessory is the only nerve immediately attached to the lateral column.

That the posterior roots of the spinal nerves are immediately attached to the posterior white columns only; and the anterior roots, to the anterior columns only; but,

That fibres from both these roots, after traversing certain portions of the grey substance, pass out again into the white columns.

That neither the anterior nor posterior white columns are connected by a transverse commissure.

That the central portion of the grey substance immediately surrounding the spinal canal is not a commissural structure, but a layer of fine fibrous tissue for supporting the walls of the canal, which is lined with a layer of columnar epithelium.

I have now stated all that I have to say at present on the subject of the spinal cord: but if the observations contained in this communication be considered by the Royal Society to throw important light on its structure, they will be followed by others that I have made on the medulla oblongata and cerebellum. I cannot conclude, however, without acknowledging the kind interest shewn me, during these inquiries, by Mr SOLLY, Mr GRAINGER, and Dr MARSHALL HALL. I am also much indebted to Mr FREDERICK BROWN (Member of the Microscopical Society) for several ingenious contrivances with which he kindly furnished me.

ART. IV.—*Analysis and Summary of One Hundred and Sixty-Two Cases of Ovariotomy or Excision of the Ovarium, which have taken place in Great Britain.* By ROBERT LEE, M.D., F.R.S., Fellow of the Royal College of Physicians, Physician to the British Lying-In Hospital, Physician-Accoucheur to the St Marylebone Infirmary, Lecturer on Midwifery and the Diseases of Women and Children, at St George's Hospital.—(Read 12th November, 1850.) (Medico-Chirurgical Transactions, Vol. xxxiv., p. 10. London, 1851).

Dr NATHAN SMITH, Professor of Physic and Surgery in Yale College, Connecticut, published, in the Edinburgh Medical and Surgical Journal for 1822, the history of a Case of Ovarian Dropsy, successfully removed by a Surgical Operation. There was a large tumour in the right side of the abdomen, which was moveable to a considerable extent, and in which a distinct fluctuation could be perceived. "The patient being placed on a bed," observes Dr Smith, "with her head and shoulders somewhat raised, an assistant pushed up the tumour to the middle of the abdomen, and held it there. I then commenced an incision about an inch below the umbilicus, directly in the linea alba, and extended it downwards three inches. I carried it down to the peritonæum, and there stopped till the blood had ceased to flow, which it soon did. The peritonæum was then divided the whole extent of the external incision. The tumour, now exposed to view, was punctured; a canula was introduced, and seven pints of a dark-coloured ropy fluid were discharged into a vessel. About one pint was lost, so that the whole was about eight pounds. Previous to puncturing the tumour, by introducing the finger by the side of it, I ascertained that it adhered for some extent to the parietes of the abdomen, on the right side, between the spine of the ilium, and the false ribs. After evacuating the fluid, I drew out the sac, which brought out with it, and adhering to it, a considerable portion of omentum. This was separated from the sac by the knife; and two arteries, which we feared might bleed, were tied with leather ligatures, and the omentum was returned. By continuing to pull out the sac, the ovarian ligament was brought out; it was cut off; two small arteries were secured as before, and the ligament returned. I then endeavoured to separate the sac from its adhesions to the parietes of the abdomen, which occupied a space about two inches square. This was effected by a slight touch of the knife, at the anterior part of the adhesion, and by the use of the fingers. The sac then came out whole, excepting where the puncture had been made, and I should think it might weigh between two and four ounces. The incision was then closed with adhesive plaster, and a bandage applied round the abdomen. No unfavourable symptoms occurred after the operation. In three weeks the patient was able to sit up and walk, and since has perfectly recovered."

This operation, similar in the first stage to the Cæsarean section, but much less formidable, was undertaken by Professor Smith, from the following considerations.

" The patient, although her health was not greatly injured, was sensibly affected by the disease. She was quite certain that the increase of the tumour in a given time was augmented, and probably, at no distant time, would destroy her. —I had also an opportunity to dissect the body of a patient who had died of ovarian dropsy, after having been tapped seven times. In this case, the sac was found to be the right ovarium, which filled the whole abdomen, but adhered to no part, except the proper ligament, which was not larger than the finger. I have seen two other ovarian sacs which were taken from patients after death; they had been tapped several times, and the sacs were equally unattached, except to their own ligaments. Thence I inferred, that in a case of ovarian dropsy, while the tumour remained moveable, it might be removed with a prospect of success; and the event has justified my expectations. The mode of operating in the above case has been described in several of my last courses of 'Lectures on Surgery.'"

In the following year, 1823, Mr John Lizars of Edinburgh, made a long incision through the abdominal parietes of a woman, aged 27, who, in the opinion of some of the most experienced physicians in that city, was afflicted with ovarian disease; but the symptoms were produced by obesity and distension of the intestines, and there was no ovarian cyst or tumour found present to remove on laying open the abdomen. This patient did not die from the operation.

In 1825, Mr Lizars removed an enlarged ovarium, by a long incision, from another patient. Some hæmorrhage followed the operation. The other ovarium was diseased, and was not removed.

In 1825, Mr Lizars operated in another case, and the results were fatal.

In 1826, Mr Lizars repeated the operation, but he encountered a vascular tumour which could not be removed.

In 1826, Dr Granville made an incision of nine inches and a half through the abdominal parietes of a woman who had an ovarian cyst; but the adhesions were so strong, that the operation was abandoned.

In 1827, Dr Granville repeated the operation, but there was no ovarian tumour to remove. Some time after this, it was proposed again to perform the operation; but the consent of the patient could not be obtained, and she died some years after under the care of Dr Scott of Stratton Street. Both ovaria were sound; and the enlargement arose from a great vascular tumour imbedded in the walls of the uterus. The preparation of the uterus and tumour is in the Museum of St George's Hospital.

The difficulty, or rather the impossibility, of determining whether ovarian cysts and tumours were present in these cases, and whether, when they actually existed, their extirpation was practicable, were strikingly illustrated by these operations; and during six years after their performance ovariotomy was abandoned in this country for the space of several years.

Between the period now mentioned, however, and 1833, the obstetrical and surgical mind of the country was not altogether dormant as to this operation. By referring to the sixty-first volume of this Journal, April 1844, it will be seen, that the experimental trials made, had led certain surgeons to modify the operation, where this modification was admissible. This consisted in imitating the practice of Dr Nathan Smith, by making an incision through the parietes of the abdomen, from one inch and a half to two inches in extent, puncturing the tumour, evacuating its contents, and then extracting, through this comparatively small opening, the cyst so contracted and reduced in size. This method was put in practice in 1833 by Mr Jeaffreson of Framlingham, who made an incision from one inch and a half to two inches long, between the *umbilicus* and the *pubes*, through the abdominal parietes of the patient, exposed an ovarian cyst, which was without adhesions, and after emptying the cyst by puncture, drew it out, applied a ligature round its peduncle, and then cut away the cyst.

This method was soon after imitated by King, West, and Phillips; and has consequently come to be one of the recognised modes of operation, when circumstances admit of its application. As it is manifest that this can be only when the cyst contains fluid and is simple; and as it is not in all cases practicable to ascertain beforehand that the contents are liquid, and that there is no more than one cyst, the method is something limited in its application.

Room was accordingly left for the adoption of the original method proposed and practised by Mr Lizars; and this, by reference to the volume mentioned, was accordingly done by Messrs Clay, Atlec, Southam, and Walne.

In 1844, it will be further seen, from the same article, the operation had been performed forty-two times for the removal of diseased *ovaria*, or for the removal of tumours which were regarded as diseased *ovaria*. In nine of these cases the tumour was not removed, or no tumour was found; and of the nine females so operated on, six recovered and three died. In the thirty-three instances in which a tumour was removed, twenty-two persons were cured, and eleven died; that is, exactly one-third.

Dr Robert Lee has given the most complete statistical account of the results of this operation hitherto made public. During the last twenty-seven years, according to his statement, the operation of ovariotomy has been attempted or performed more than one hundred and sixty times in Great Britain. Of one hundred and sixty-two cases, he has obtained written reports. In sixty of these cases, the ovarian disease could not be removed; in nineteen of these sixty, death took place. Of the residual hundred and two cases in which the operation was completed, forty cases terminated fatally. The present condition of the sixty patients who recovered is very imperfectly known.

In about one-third of the whole number, the analysis shews that before an opening had been made into the sac of the peritonæum, it was impossible to determine whether any ovarian disease actually existed; or whether, when ovarian cysts and tumours were present, their relations and characters were such that it was possible to extirpate them by surgical operation.

Analysis of 162 *Cases of Ovariotomy which have occurred in Great Britain.*

1.—Oct. 24, 1823. Aged 27. Enlargement commenced 1815. Long incision. No ovarian cyst or tumour; the enlargement produced by obesity and distension of the intestines. (Observations on Extraction of Diseased Ovaria. J. Lizars, Esq. Edinburgh, 1825.) No ovarian disease to be removed. Recovered. Op. J. Lizars, Esq.

2.—Feb. 27, 1825. Aged 36. One enlarged ovarium removed by a long incision; the other ovarium diseased and not removed. Some hæmorrhage followed the operation. The ligature had not come away when the history of the case was published. (Ibid.) Removed. Recovered. Op. J. Lizars, Esq.

3.—March 22, 1825. Aged 25. Ovarian tumour seven pounds, adhering to the brim of the pelvis, colon, and abdominal walls, which were partly overcome by dissection, tension, and the handle of the scalpel. Incision from sternum to *symphysis pubis*. Gangrene of the intestine followed. (Ibid.) Removed. Died. Op. J. Lizars, Esq.

4.—1825. Aged 34. A solid and vascular fibrous tumour of the uterus, adhering to the *omentum majus*. (This patient died in 1850. Both ovaria were found healthy. The fibrous tumour adhered to the *fundus uteri.*) (Ibid). No ovarian disease to be removed. Recovered. Op. J. Lizars, Esq.

5.—Date and age not stated. A case similar to No. 1 was seen by Dr Gooch about 1829 in Guy's Hospital. The patient, a young woman, had been in the hospital some time before, for what was considered to be ovarian dropsy; but under the use of purgatives the tumour disappeared. A few months before Dr Gooch saw her, the swelling having returned, she consulted a surgeon, who assured her that she had a tumour in the ovary which could be removed only by excision. For this purpose, he made in the *linea alba* an incision six inches long, by which the cavity of the abdomen was exposed. It was then discovered, as in the case at Edinburgh, that there was no tumour, and that the enlargement depended on flatulence and fat. The wound was closed and healed; but the patient's health sustained great injury. (See Dr Gooch's Account, page 222). No ovarian disease to be removed. Recovered. Operator unknown.

6.—1826. Age not stated. Incision nine inches and a-half long; brought to view a large ovarian tumour, which had extensive adhesions, by firm bands, with the adjacent viscera and peritonæal coverings. (Medical Gazette, vol. i., 1842–3, p. 340). Not removed. Recovered. Op. Dr Granville.

7.—Mar. 21, 1829. Aged 30. Incision nine inches; a tumour, which weighed eight pounds, removed, and supposed to be ovarian. Dr Lee examined this tumour in the recent state, and ascertained that it was a fibrous tumour, which had adhered to the *fundus uteri* by a thick peduncle. A portion of small intestine, which had come in contact with this root where incised, became inflamed and gangrenous. The preparation was several years after in the possession of J. North Esq., but it has not been preserved. No ovarian disease to be removed. Died. Op. Dr Granville.

8.—1835. Age not stated. November 1833, a tumour, discovered during the second labour, occupying the entire left half of the pelvis, pressed above the brim of the pelvis, and the labour natural. March, 4, 1845, delivered again without assistance. Left ovarium dropsical, which afterwards rapidly increased. Incision from ten to twelve lines in the course of the *linea alba*, between umbilicus and pubes. One cyst containing twelve pints, and another two ounces. The entire ovary extracted, and its connections divided. Ends of ligature protruded. Wound closed with two sutures, adhesive plaster, and lint. On the 10th, incessant vomiting, hiccough; pulse scarcely to be felt. These symptoms subsided. Secretion of milk. The patient recovered; and has since given birth to five healthy children, and is still alive. Removed. Op. W. Jeaffreson, Esq.

9.—Date and age not stated. Abdomen distended during three years with solid and fluid matter. Two gallons of fluid removed by tapping. Operation repeated. The solid part of the mass was large, irregular, and presenting three spherical projections. Elastic feel. The disease had made slow progress. An incision about three inches through the parietes, a little above a line drawn across the abdomen from the umbilicus. The fluid escaped, and a portion of omentum. The wound was closed. Died a few months after.

The tumour, after death, was found to be an enlarged gland, in a fold of the mesentery. (Lancet, vol. i., 1837, p. 586). Not removed. Op. H. C. King, Esq.

10.—Mar. 1834. Age 40. A tumour on the right side, between the superior spine of the ilium and the short ribs, four to five inches long, and two or three broad. Moveable. Health not much deranged. A vertical incision through the parietes and *linea semilunaris* of seven or eight inches. Search made for tumour without success. Wound enlarged four inches, in direction of lumbar vertebræ. Tumour not discovered. Wound closed. Health improved after the operation. (Lancet, vol. i., 1837, p. 587). Not removed. Recovered. Op. H. C. King, Esq.

11.—1836. Hannah Cavell, aged 40. Aug. 1833. Abdomen enlarging for three years. Enlargement, with fluctuation, went on increasing. Emaciation. An incision as in the last case. Twenty-seven pints of gelatinous fluid removed. Opening enlarged to three inches. A large thick cyst and solid tumour extracted. Ligature slipped off. Three ligatures reapplied. Little hæmorrhage. The patient was in good health five months after, and twelve months since. Op. H. C. King, Esq.

12.—Nov. 1836. Mrs Hurrion, of Southborough, aged 45, mother of three children. Incision, two inches. Sac punctured, and twenty pints of fluid drawn off; then drawn out, and its root tied and divided. No adhesions. Doing well when the history of the case was published. Present, Dr Scudamore and Mr Hargraves. This patient is now alive and well. (See the Tables of Mr B. Phillips and Mr S. Lee). Op. —— West, Esq.

13.—Date not stated. Miss S., aged 23. Small incision. No adhesions. Twenty-four pints of fluid removed. (Ibid). Recovered. Op. —— West, Esq.

14.—Date not stated. Aged 40. Small incision. Adhesions. (Ibid). Cyst not removed. Recovered. Op. —— West, Esq.

15.—Date not stated. Aged 24. Short incisions. Cyst tapped before extraction. Eleven gallons of fluid removed. (Ibid). Died. Op. —— West, Esq.

16.—Date and age not stated. The disease had existed more than twenty years. Tapping twice. No details of the operation could be obtained. The lady recovered perfectly, and was then in good health. (Lancet, 1839–40, vol. i., p. 287). Removed. Op. B. C. Crisp, Esq.

17.—Date and age not stated. "On discharging the fluid (about five pints of dark grumous matter) the sac was found to be very thickened and adherent, and complicated with tumour, about the size of a child's head at birth. The patient died in about five days. A *post-mortem* was not allowed." (Communicated by Mr Hargraves, and never previously published). Not removed. Op. T. Hargraves, Esq.

18.—Date and age not stated. "I was also present at an unsuccessful case at Riverhead, where the disease was complicated with tumour. The patient died in less than a week after the operation, although little more was done than the fluid let off, and a slight attempt made to break through the adhesions to the parietes." (Communicated by Mr Hargraves, and likewise never previously published). Not removed. Op. ——

19.—1840. Aged 21. Unmarried. In good general health. Incision two inches and a-half. Sac seized with the vulsella. 330 ounces of fluid evacuated. Opening enlarged. Cyst drawn out. Root tied and excised, and sac removed without difficulty. Severe pain followed, with vomiting; and I saw the patient about half an hour afterwards with rapid feeble pulse, and cold extremities. I was present at the examination of the body, when the appearances of recent inflammation were observed within the pelvis, with a small quantity of extravasated blood. St Marylebone Infirmary. (Med. Gazette, vol. xxvii. p. 83). Died. Op. B. Phillips, Esq.

20.—1843. E. D., aged 19. Fifteen months before slight increase of abdomen. Perceived pain in right side, with fever. Rapid increase. A large tumour occupying the abdomen. On the left side adhesions suspected where the mass was heard. Fluctuation on right side. Incision at first four inches below umbilicus. Incision extended upwards to the ensiform cartilage. No adhesions. (Guy's Hospital Reports, vol. i., p. 477). Removed. Died. Op. A. Key, Esq.

21.—1843. Aged 32. Married, but never pregnant; cysts and solid tumour. Adhesions, with cancerous disease of the uterus. Long incision. A portion of omentum included in ligature. Peritonitis. This and the last case occurred in Guy's Hospital. (Med.-Chir. Trans., vol. xxvii., p. 76). Removed. Died. Op. B. Cooper, Esq.

22.—Sept. 3, 1843. Mary Nicholson, married, aged 29. Good health till her marriage, more than two years before. Six months after a moveable tumour, the size of an orange, in the pubic region, which rapidly enlarged. Nine months after supposed to be in labour. Abdomen enlarged, and strength failed. Fluctuation in one or two places, but the tumour generally firm. Incision from near the ensiform cartilage to the pubes. Several adhesions divided. Root tied, and tumour excised. (Med.-Chir. Trans. vol. xxvii., p. 88). Removed. Died. Op. T. M. Greenhow, Esq.

23.—Nov. 19, 1843. Miss —, aged 28. Removed the entire cyst. No adhesions. Had suppurations within the peritonæal cavity. Recovered. Since married, has had one dead and one living child. Is now living. (The history of this and the following ten cases was communicated by Mr Lane to Dr Lee). Removed. Op. S. Lane, Esq.

24.—Feb. 15, 1844. Mrs L., aged 47. Removed a very large cyst. Firm adhesions to the anterior wall of abdomen above the umbilicus, and from one hypogastric region to the other. None elsewhere. Recovered. Died two years after of stricture of the rectum. (Ibid.) Op. S. Lane, Esq.

25.—Feb. 15, 1844. Mrs —, aged 43. Removed the cyst, with the exception of a portion attached to the side and fundus of the uterus. Recovered. Lost her husband three years after the operation. Married again in about a year. Died about fifteen months after her last marriage. (Ibid.) Op. S. Lane, Esq.

26.—Nov. 21, 1844. Miss P., aged 20. Removed the cyst. No adhesions. Recovered. Now living. (Ibid.) Op. S. Lane, Esq.

27.—Sept. 1845. Mrs W., aged 40. Removed the cyst. Universal adhesions to the anterior wall of the abdomen; also posteriorly to the liver, kidney, renal capsule, and the cava inferior. Left a small portion of the cyst adherent to the renal capsule and cava. Died of peritonitis on the third day from the operation. (Ibid.) Op. S. Lane, Esq.

28.—1845. Miss T., aged 39. Removed the cyst. No adhesions. Had pelvic abscess. Recovered. Still living, but the discharge from the abscess has never ceased; and the movements of the hip-joint are much interfered with. (Ibid). Op. S. Lane, Esq.

29.—Nov. 1846. Miss A., aged 31. Incision made. Attempts made to remove the cyst not successful. Recovered rapidly from the operation. Died two years afterwards from an attempt to produce suppuration of the adhering cyst. (Ibid). Op. S. Lane, Esq.

30.—April 24, 1847. Mrs P., aged 40. Solid tumours and ascites. Small explorative incision made. Tumours not removed on account of their connections. Well in two days from the operation. Still living. (Ibid.) Op. S. Lane, Esq.

31.—Date and age not stated. Large solid tumour of the size of the uterus at the full period of pregnancy. Incision from umbilicus to pubes. Tumour too much connected with the uterus for removal. Recovered from the operation. Wound healed. Able to walk about. Died suddenly five weeks from the operation, after an attack of dyspnœa. The *post-mortem* examination discovered no cause of death, with the exception, perhaps, of pale flabby heart. No evidence of inflammation of the abdominal cavity. (Ibid.) Tumour not removed. Op. S. Lane, Esq.

32.—Oct. 15, 1848. Miss D., aged 54. Incision made. Universal adhesions prevented the removal of cysts. Rapid recovery from the operation. Two years after, suppuration produced in three or four of the cysts. Now living in tolerable health. (Ibid.) Op. S. Lane, Esq.

33.—Nov. 1849. Miss H., aged 24. Inflammation of the cyst produced, followed by cure. (Ibid). Not removed. Op. S. Lane, Esq.

34.—1839. Aged 26. Small incision. Adhesions. Tumour not extracted. Operated upon in Guy's Hospital by Mr Morgan. Considered by all who saw the case most favourable for the operation. (See Mr B. Phillip's Table, p. 474). Died. Op. — Morgan, Esq.

35.—1842. Aged 46. The tumour large and solid. Pelvic cavity filled. Fluctuation one, detected above the pubes. Incision nearly from sternum to pubes. (The histories of this and the following forty-five cases were communicated by Dr Clay.) Removed. Recovered. Op. Dr Clay.

36.—1842. Aged 57. Nine children. Ascites. Paracentesis. An ovarian tumour four pounds in the left iliac region, hard and heavy. Parietes in front, below the umbilicus, attached to it. Incision ten inches. Strong adhesions in every direction, overcome by the scalpel. A double ligature passed through the root. Tumour removed. Recovery rapid. (Ibid.) Op. Dr Clay.

37.—1842. Aged 47. Great enlargement, and fluctuation chiefly on the left side. Two years before tapped of two pints of bloody fluid. At first a small incision. The whole anterior surface of the mass adherent. Tumour vascular. Pierced in various parts, when blood issued. The removal of the tumour impracticable. Died on the sixth day after the operation. (Ibid.) Op. Dr Clay.

38.—1842. Aged 39. Right side of abdomen enlarged four years before. Tapped four times. A bold incision. Sac exposed with numerous incisions. An immense sac removed. Recovery rapid. Good health. (Ibid). Op. Dr Clay.

39.—1843. Mrs L., aged 35. Frequently tapped. Extensive adhesions between the cyst and parietes. Tumour eighteen pounds and a-half. Fluid seven pounds. (Ibid.) Removed. Died. Dr Clay.

40.—1843. Mrs E., aged 40. Steril. The tumour removed weighed thirty pounds. Peritonitis in twelve hours; and she died at the close of the second day. (Ibid). Op. Dr Clay.

41.—1843. Mrs B., aged 46. Forty pounds of fluid drawn off by tapping. Numerous small tumours felt. Incision four inches. Large quantity of fluid with hydatids escaped. Most of abdominal viscera enlarged and adhering together, especially the uterus; right ovary and spleen. Twelve folds of white worsted a foot in length introduced into the abdominal cavity. Wound closed. Two years after in health. Not removed. Op. Dr Clay.

42.—1843. Mrs P., aged 40. Ovarian disease of ten or twelve years' standing. Repeatedly tapped. A large solid mass with the empty sac right side. Incision ten inches. Three adhesions. Active inflammation followed, but was subdued. Removed. Recovered. Op. Dr Clay.

43.—1845. Mrs W., aged 38. Large ovarian tumour. Frequent attacks of inflammation. Incision four inches. Extensive adhesions. Cleared out sac. A string of threads left hanging out of it. Discharge nearly gone in four months. Size of abdomen nearly natural. In 1847 in good health. Not removed. Recovered. Dr Clay.

44.—1846. Mrs S., aged 51. Abdomen, size of the gravid uterus at the full period. Tapped. Sac extirpated. Incision from umbilicus to pubes. No adhesions. No unfavourable symptoms followed. Removed. Recovered. Op. Dr Clay.

45.—1845. Mrs T., aged 35. Steril. Tapped thrice. Solid part moveable. Few adhesions. Length of incision not stated. Removed. Recovered. Op. Dr Clay.

46.—1845. Aged 38. Two ovarian cysts. Tapped. Two solid masses remained, and adhesions in two places to the parietes. Length of incision not stated. Cysts and solid matter removed. Six months after was stouter than at any former period of life. Op. Dr Clay.

47.—1843. Mrs H., aged 45. Tumour size of the gravid uterus at the eighth month, hard, unyielding, lobulated, moveable, not fluctuating. Incision thirteen inches. No adhesions. Tumour exposed. Has a broad attachment. The greater part of the uterus forming part of the tumour. Great difficulty in securing the exposed vessels. Attacks of syncope succeeded, and she died in one hour and a-half. The tumour weighed twelve pounds, and included a portion of the os and cervix uteri. Removed. Op. Dr Clay.

48.—1844. Aged 52. Enormous enlargement of abdomen of sixteen years' duration. Incision twelve inches. Uterus enlarged to twenty pounds. Left ovary four pounds. The whole extirpated, converting the vagina into a *cul de sac.* Both ovaries and uterus extirpated. Died on the fifteenth day. Op. Dr Clay.

49.—1846. Aged 26. A large ovarian cyst of three years' duration, with a small solid mass. First tapped, and then the cyst extirpated. Weight thirty-five pounds. No adhesions. Died on the tenth day. Op. Dr Clay.

50.—1846. Aged 32. One child, seven years before; began to enlarge in 1842. Tapped five times. Incision thirteen inches. Recovered perfectly in less than five weeks, and is now perfectly well. Removed. Op. Dr Clay.

51.—1848. Aged 27. Ovarian disease of three years' duration. Tapped. On opening abdomen, a sac with few adhesions, exposed, except that near the attachment to the right ovary, was a solid mass of five pounds, so firmly attached to the adjacent parts that its removal was improper. The sac and its contents removed. A thick tent passed into the substance of the solid mass. In five weeks, tent came away, and discharge ceased. No swelling now remains, and she has resumed her occupation. Op. Dr Clay.

52.—1847. Mrs H., aged 32. Tapped. After, in the right iliac region, a solid mass of four or five pounds, without adhesions. Tapped. Operation easy. No adhesions. The sac and mass removed, fourteen pounds and a half. Became pregnant five months after. Delivered of a dead fœtus. Is now in good health. Op. Dr Clay.

53.—1846. Aged 22. Ovarian disease on the right side for five or six years. A large sac tapped six times, the last time a few days before the operation. A broad patch of adhesions. Tumour weighed twenty-nine pounds. Removed. Recovered. Op. Dr Clay.

54.—1846. Aged 35. Tapped six times. Tumour of considerable size, without adhesions. Incision fourteen inches. On the fifteenth day after the operation returned home. Removed. Recovered. Op. Dr Clay.

55.—1846. Mrs A., aged 45. Ovarian disease twelve years. Large solid tumour. Incision from sternum to pubes. Adhesions. Removed. Peritonitis followed, but she recovered. Op. Dr Clay.

56.—1847. Mrs J., aged 51. Emaciated. Ovarian discase sixteen years. Tumour weighed forty pounds. Some adhesions. Died in thirty-six hours. Op. Dr Clay.

57.—1848. Mrs L., aged 51. Long standing ovarian disease. Tapping. A cyst with adhesions, and a solid mass on the right side. Incision ten inches. Recovered, and was in good health in less than a month. Op. Dr Clay.

58.—1848. Mrs B., aged 47. Ovarian disease five or six years. Tapped. Sac adherent. Exploratory incision. No further proceeding. An incision lower down. No adhesion detected. Extirpation. Exhaustion. Died on the sixth day. Not removed. Op. Dr Clay.

59.—1848. Mrs B., aged 40. Tumour removed, forty-six pounds. Now in excellent health. Op. Dr Clay.

60.—1848. Miss M. K., Edinburgh, aged 19. Tumour removed. Died within twenty-four hours. Op. Dr Clay.

61.—1848. Mrs T., aged 35. Tumour removed, which, with cystic and ascitic fluid, weighed fifty pounds. Died. Op. Dr Clay.

62.—1846. Ellen D., aged 27. Tumour removed, weighed forty-eight pounds. Now quite well. Op. Dr Clay.

63.—1848. Mrs S., aged 45. Tumour removed, which weighed twenty-eight pounds. Now in good health. Op. Dr Clay.

64.—1847. Mrs A., aged 25. Tumour removed, forty pounds. Is now quite well. Op. Dr Clay.

65.—1848. Miss J., aged 18. Tumour removed, thirty pounds. Died in thirty-six hours. Op. Dr Clay.

66.—1848. Sarah J., aged 47. Tumour removed, thirty-seven pounds. Now well. Op. Dr Clay.

67.—1847. Aged 27. Tumour removed, thirty pounds. Died on the ninth day. Op. Dr Clay.

68.—1848. Mrs R., aged 35. Tumour removed, twenty pounds. Died on the third day. Op. Dr Clay.

69.—1846. Mrs M'A., aged 37. Tumour removed, forty pounds. Recovered very slowly, but is now well. Op. Dr Clay.

70.—1849. Mrs W., aged 33. Weight of tumour thirty-one pounds. Is now well, and has had a still-born child since. Op. Dr Clay.

71.—1849. Miss W., aged 32. Tumour thirty-five pounds. Now in good health. Op. Dr Clay.

72.—1849. Mrs S., aged 48. Removed tumour, seventy-six pounds. Recovered. Now in good health. Op. Dr Clay.

73.—1850. Mrs H., aged 45. Weight of tumour extirpated, twenty-four pounds. Recovered. Now quite well. Op. Dr Clay.

74.—1850. Mrs S., aged 38. Weight of tumour, twenty-four pounds. Contents of sac two days previously, thirty pounds. Now proceeding favourably. Removed. Recovered. Op. Dr Clay.

75.—1844. H. H., aged 22. Ovarian disease five or six years; tapped six times; tumour twenty-nine pounds fourteen ounces; long incision. Not removed. Recovered from the operation. Op. Dr Clay.

76.—Date and age not stated. Four cases of peritonæal sectional exploration from one inch and a half to two inches and a half. Not removed. Recovered. Op. Dr Clay.

77.—Date and age not stated. Second case of peritonæal sectional exploration. Not removed. Recovered. Op. Dr Clay.

78.—Date and age not stated. Third case of peritonæal sectional exploration. Not removed. Recovered. Op. Dr Clay.

79.—Date and age not stated. Fourth case of peritonæal sectional exploration. Not removed. Recovered. Op. Dr Clay.

80.—Date and age not stated. Fifth case of peritonæal sectional exploration. Not removed. Died. Op. Dr Clay.

81.—Oct. 20, 1843. Mrs H., aged 37, mother of six children. After a protracted labour two years and a half before, followed by inflammation, a small tumour felt above and to the left side of the pubes. Abdomen afterwards increased with pain. Before the operation, above the pubes, was a circumscribed moveable tumour the size of the fœtal head. Uterus healthy. Incision through the skin from the right of the umbilicus along the mesial line to within three inches of *symphysis pubis.* Peritonæum exposed and punctured. Four quarts of ascitic fluid escaped. Wound enlarged to the extent of nine inches. Omentum adhering to tumour, separated. Pedicle of tumour tied and removed. Tympanites and obstinate vomiting, which caused the ends of the ligatures to be drawn within the abdominal cavity. After several weeks a small abscess at the lower part of the cicatrix. In 1845 the patient was in perfect health. (Med. Gaz., A. 1843.) Removed. Recovered. Op. G. Southam, Esq.

82.—June 24, 1845. Mrs S., aged 38, married twenty years. Steril. Enlargement observed eight years before. Abdomen before the operation greatly enlarged. Fluctuation of a resisting kind. Exploratory incision. No adhesions. Cyst punctured. Sixteen pints of fluid evacuated. Opening enlarged above and below to six or seven inches. Cyst drawn out. Root tied and divided. The ligature came away on the forty-ninth day, and the patient is now in the enjoyment of perfect health. (Provincial Med. and Surg. Association, 1845). Recovered. Op. G. Southam, Esq.

83.—May 21, 1847. Mary H., aged 26. Five children. After last confinement became dropsical, and was tapped. Tumour has recently increased rapidly with pain. Incision first made three inches long. Six pounds of ascitic fluid discharged. No adhesions. Attempt to tap the tumour not successful. Incision extended; adhesions. Tumour removed. Faintness. Dyspnœa took place on the 23d; and death on the 26th. (Provincial Medical and Surgical Journal, new series, vol. iii., 1847). Op. G. Southam, Esq.

84.—Date and age not stated. "In another case I made an exploratory incision," says Mr Southam, "with a view of determining on the propriety of extirpation, or adopting the operation recommended by Mr Bainbrigge of Liverpool, having previously satisfied myself that adhesions existed, which I thought, if they proved too extensive in extirpation, might facilitate the other operation. The patient recovered from the exploratory incision without a bad symptom, but died from the effect of Mr Bainbrigge's operation, suppuration of the cyst having produced violent irritative fever. The tumour was multilocular, consequently a very favourable one for Mr Bainbrigge's plan." (This case is not published.) Op. G. Southam, Esq.

85.—1846. Aged 20. Unmarried. Tumour first perceived eighteen months before. Tapping six times, large incision. Both ovaria diseased and removed. Fœtal peritonitis and phlebitis followed. (Edinburgh Medical and Surgical Journal, vol. lxv., pp. 278—308.) Both ovaria removed. Died. Op. Dr Handyside.

86.—September 3, 1846. Mrs P., aged 38. Married, and five children. Seen first by Dr Handyside, October 1845, eleven months subsequent to the birth of her last child. A moveable tumour of the left ovary, with a large dropsical swelling. February 1846, tumour and dropsical swelling increased. Tapping. The tapping repeated thrice. September 3, "Incision four inches. A similar procedure adopted to that in the last case, with the exception of the line of exit for the ligature being through the recto-vaginal cul-de-sac of peritonæum." "The two ligatures were carried out per vaginam." The tumour, consisting chiefly of cysts, weighed ten pounds. (Dr Handyside has communicated the history of this and the following case.) Both ovaria removed. Died of peritonitis. Op. Dr Handyside.

87.—Dr Simpson communicated verbally to the Edinburgh Medical and Chirurgical Society, in December 1849, a notice of a fatal case of ovariotomy that occurred to himself. So far as Dr H. knows, this is the only instance in which he has operated. (Unpublished). Died. Op. Dr Simpson.

88.—1848. Aged 23. An incision first made of one inch, the finger introduced, and no adhesion felt. Opening extended to three inches. The sac tapped. A second cyst detected and tapped, both cysts firmly adhering. Operation could not be completed. (Middlesex Hospital Pathological Transactions.) Not removed. Died. Op. J. M. Arnott, Esq.

89.—1849. F. W., aged 24. Abdomen first perceived to be swollen in 1847. Tapped in operation July 9, 1849. Incision twelve inches. Compound cyst and solid tumour. One adhesion. Pedicle thick as a middle-sized finger. August 15, wound healed. (Medical Gazette, vol. xliv., p. 366.) Removed. Recovered. Op. J. Crouch, Esq.

90.—1843. Aged 40. Long incision. Fibrous tumour of uterus removed. Hæmorrhage. (See S. Lee's Table, p. 268). Died. Op. A. M. Heath, Esq.

91.—Date and age not stated. Incision. Cyst removed. Recovered. (Ibid.) Op. M. Morris, Esq., Rochdale.

92.—1846. Aged 24. This patient, seen first by Mr Arrowsmith, April 1846, had been tapped once for ovarian dropsy. The case in all respects appeared favourable for the operation. An incision six inches. Adhesions so strong in every direction, that the farther steps of the operation were abandoned; no bad symptoms followed. Some weeks after was tapped. The further history of the case unknown. (Communicated by Mr Arrowsmith to Dr Lee, June 4, 1851). Not removed. Recovered. Op. J. Y. Arrowsmith, Esq.

93.—1846. Aged 18. St George's Hospital. A single cyst without adhesions. Incision three inches. (Medical Gazette, vol. iii., New Series, p. 735.) Removed. Recovered. Op. C. Hawkins, Esq.

94.—1846. Aged 20. Incision four inches. No adhesions. One cyst, right side. Hæmorrhage from slipping of the ligature. Left ovarium diseased. (Medical Gazette.) Died. Op. S. Solly, Esq.

95.—1846. Aged 39. Large incision. No adhesions. Multilocular cyst, complicated with chronic peritonitis. Tumour weighed twenty pounds, fluid ten pounds. Died in four hours from shock of operation. (See S. Lee's Treatise, p. 271.) Removed. Op. Dr P. Smith.

96.—1845. Aged 18. Unmarried. Duration of disease eighteen months. Incision fourteen inches. Adhesions. Two pints of fluid drawn off. The two hands introduced on each side of the tumour deep into the pelvis, and tumour raised. A double ligature passed through the ligature and divided. (Prov. Med. and Surgical Journal, 1845, p. 597.) Removed. Recovered. Op. J. Dickens, Esq.

97.—1846. Aged 25. Married six years. Three children. Enlargement first observed seven years before. Small incision first made. No adhesions. The incision extended up to within one inch and a half of the ensiform cartilage and downwards to the pubes. Cyst tapped and drawn out. Uterus six months gravid. Cysts and solid matter removed. Afterwards became pregnant, and had a healthy child. (Medico-Chirurgical Transactions, vol. xxx., p. 95.) Recovered. Op. H. E. Burd, Esq.

98.—1850. Aged 38. Compound cysts of seven or eight months' duration. Incision under three inches. Some difficulty in extracting the large cyst. Tapped, and difficulty overcome. Pedicle tied and divided. (Ibid., vol. xxxiv., pp. 1—3.) Recovered. Op. E. Duffin, Esq.

99.—February 19, 1850. Aged 19. September 1848, a tumour the size of an orange on the right side of the hypogastrium first perceived, which gradually increased in size. In January 1850, the abdomen as large as at the full period of gestation. Fluctuation distinct. Incision ten inches. Two slight adhesions between the omentum and anterior surface of tumour. Cyst emptied. Root tied and divided. One large and several small cysts removed. The patient in good health eight months after. (Lancet, vol. ii., 1850, p. 680.) Op. C. H. Cornish, Esq.

100.—March 7, 1850. Aged 33. September 17, 1844, tapped. Treatment by pressure twelve months. Married, and safely delivered within a year. Tapped 1846 and 1850. An exploratory incision made. A slight recent adhesion. Incision extended to twelve inches. An enormous triple-headed cyst removed. Died on the third day from peritonitis. (History communicated by Dr Ely and Mr Isaac Brown to Dr Lee, and not previously published.) Op. I. Brown, Esq.

101.—1850. Aged 24. Married nine months in February 1850. Speedy pregnancy. Abdomen large. Labour natural. Abdomen continued large. Constitutional disturbance. Jaundice. Ten days after delivery abdomen forty-eight inches and a half in circumference. Eleven weeks after delivery seven gallons of dirty brownish fluid, with flakes of lymph and pus, drawn off. No bad symptoms followed. but twenty-five days after, abdomen measured fifty inches. Operation then performed. Incision three inches a little to the left side. Finger passed between two closely adherent layers, which were the parietal peritonæum and the sac closely adherent. Mr Phillips seized the exposed sac with double hooks; but it was rent, and the contents escaped, which were five gallons of pure pus. Edges of sac fastened to wound of abdominal walls. Died ninety-six hours after the operation. After death, cyst found universally adherent. (History communicated by Dr West and Mr Paget to Dr Lee, and never previously published.) Op. J. Paget, Esq.

102.—September 2, 1848. Susan C., aged 34. Married nine years. One child. Ovarian disease two-and-a-half year's duration. Tapped twice. Emaciated and debilitated. Dyspnœa. Incision from ten to twelve inches. Extensive adhesions. The attempt at its removal abandoned. Eight days after wound nearly closed. Cyst fast filling. Six days after, during a fit of coughing and vomiting, wound burst asunder and many pints of fluid escaped, and the flow continued till she died on the 23d, three weeks after the operation. A second cyst. The other ovarium had a single cyst attached, from which three pints of fluid escaped. (Communicated by Mr Anderson to Dr Lee, and never previously published.) Op. A. Anderson, Esq.

103.—October 29, 1849. Aged 25. Unmarried. At the age of 22, tumour appeared in right iliac region; increased rapidly during last six months, but its development unattended with pain. Universal fluctuation. Measured forty-one inches in circumference. Cyst presumed to be single and not adherent. Operation by small incision resolved on. Cyst found to be of remarkable tenuity, containing transparent fluid. Contrary to expectations universal adhesions to peritonæum and compound cysts. In fourteen days the abdomen as large as before. (Communicated by Dr West to Dr Lee, and never before published.) Not removed. Recovered. Op. C. De Morgan, Esq.

104.—1845. Aged 33. Incision four inches. No adhesions. Cysts tapped and extracted. (See S. Lee's Table, p. 270.) Removed. Recovered. Op. W. B. Page.

105.—Date and age not stated. Long incision. Adhesions. (Mr Phillip's Table). Not removed. Died. Op. A. B.

106.—Date and age not stated. Small incision. Adhesions. (Ibid.) No tumour. Recovered. Op. C. D.

107.—Large incision. Adhesions. (Ibid.) Died. Op. E. F.

108.—Date and age not mentioned. Long incision. Extensive and strong adhesions which were violently torn up. A personal friend who was present at the operation communicated all the details to Dr Lee. Death speedily followed. (Unpublished.) Removed. Op. Mr Walne.

109.—Date not stated. Aged 58. Large incision. No adhesions. Tumour sixteen pounds, nearly all fluid. (Mr B. Phillip's Table, Med.-Chir. Trans., vol. xxvii., p. 474.) Recovered. Op. Mr Walne.

110.—Date not stated. Aged 57. Large incision. No adhesions. Tumour sixteen pounds and three-quarters. (Ibid.) Recovered. Op. Mr Walne.

111.—Date not stated. Aged 20. Large incision. No adhesions. Tumour twenty-eight pounds. (Ibid.) Recovered. Op. Mr Walne.

112.—Date not stated. Aged 54. Large incision. Adhesions. Not extracted. (Ibid.) Recovered. Op. Mr Walne.

113.—Date not stated. Aged 45. Large incision. No adhesions. Tumour twenty-four pounds. (Ibid.) Died. Op. Mr Walne.

114.—April 22, 1844. Miss D., aged 30. Abdomen enlarged from ovarian disease, January 1841. In January 1844, consulted Mr Walne, who proposed the operation for removal. Operation for removal commenced by Mr Walne making an incision of one inch midway between umbilicus and pubes in *linea alba*, down to peritonæum. Dr Blundell inserted a silver probe its full length into the cavity of the abdomen without ascertaining the position of the solid tumour. Operation proceeded with. Incision extended to about three inches in length, and fluid contents of sac burst forth. About twenty-four pints of fluid ran off, leaving the integuments flaccid, and the size entirely reduced. Further progress of operation stopped. Patient placed in bed, when for nearly a week she suffered much from fever and cerebral disturbance. The patient recovered, but abdomen became again full. In 1846 married. In 1847 bore a still-born child. In 1849 bore a healthy child, which with herself continues well up to the present time; October 31, 1850. Mr Walne, Dr Blundell, Mr Vincent, and Dr Hogg, were present on the occasion.

Dr Hogg communicated to Dr Lee the above details of this case, which have not hitherto been published; Mr Walne having declined to communicate to Dr Lee the entire results of his operations, successful and unsuccessful, after repeated solicitations to do so. It is impossible, therefore, to state these results, as they have not yet been published.

Dr F. Bird likewise declined communicating the desired information, but has since published the following imperfect details.

115.—Date and age not stated. Mrs G. Large, non-adherent tumour. (Lancet, vol. ii., 1850, p 592.) Removed. Recovered. Op. Dr F. Bird.

116.—Date and age not stated. Miss H. Large non-adherent tumour. Sessile. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

117.—Date and age not stated. Mrs W. Large colloid tumour, generally adherent. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

118.—Date and age not stated. Miss M. Large simple sac. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

119.—Date and age not stated. Tumour adherent to pelvis and uterus, rendering necessary to leave a segment of the cyst attached. (Ibid.) Removed partially. Recovered. Op. Dr F. Bird.

120.—Date and age not stated. H. T. Tumour adherent to pelvis, of great thickness. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

121.—Date and age not stated. Miss M. Large sessile tumour. Adhesions. (Ibid.) Removed. Died. Op. Dr F. Bird.

122.—Date and age not stated. Mrs L. Tumour bound down in pelvis, causing unceasing suffering. Tapping required every ten or twelve days. (Ibid.) Removed. Died. Op. Dr F. Bird.

123.—Date and age not stated. Mrs H. Two large compound tumours, involving both ovaries. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

124.—Date and age not stated. Mrs G. Small tumour, non-adherent. (Ibid.) Removed. Died. Op. Dr F. Bird.

125.—Date and age not stated. Large compound tumour. (Ibid.) Removed. Died. Op. Dr F. Bird.

126.—Date and age not stated. Miss K. Large, and slightly-adherent tumour. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

127.—Date and age not stated. Large tumour, with very short pedicle. (Ibid.) Removed. Recovered. Op. Dr F. Bird.

128.—January 6, 1848. Mrs P., aged 30. Very large malignant mass. Inseparably adherent posteriorly. Extreme suffering from distension by solid matter, and rapidly approaching death, rendered the attempted operation justifiable. (Ibid.) The following details of this case have been communicated to Dr Lee by Dr Hogg.

Mrs P. married in 1841. Never pregnant. January 1844, first perceived enlargement of the abdomen. In 1846 had strong pressure applied to the abdomen, after being tapped by Mr J. Brown. A second tapping. The operation of ovariotomy undertaken by Dr F. Bird, 6th January 1848; present Dr Rigby, Mr Holt, two assistants, and Dr Hogg, who has communicated these details to Dr Lee. "An incision of two inches was made in the *linea alba*, midway between the umbilicus and pubes, and twenty pints of liquid were drawn off by a large canula; the opening was then fairly made into the cavity of the abdomen, and the solid tumour seized by forceps; the size, however, of the tumour was such, that the opening was of necessity extended to ten inches before it could be drawn forth. Tumour then found adhering strongly behind the small intestines, and, in fact, to all the abdominal viscera. Dr F. Bird attempted to separate it from them, by conveying his hand behind it, and, to a certain extent, succeeded in so doing; but on the tumour advancing through the opening, it brought the colon with it, rather before it, which was so firmly adhering that it defied all attempts at separation, even with the handle of the scalpel; the removal being impossible, it was replaced in the abdomen, and the external opening sewed up. The operation occupied one hour and ten minutes, during the whole of which time the patient was kept under the influence of chloroform. The patient, on recovering herself from the effects of the chloroform, stated that she had heard all that was said, but suffered very little. She expired twenty-eight hours after." (Unpublished.) Op. Dr F. Bird.

129.—Date and age not stated. Mrs P. Small incision; then tapped. (Ibid.) Not removed. Is now living. Op. Dr F. Bird.

130.—Date and age not stated. Small incision; then tapped. (Ibid.) Not removed. Is now living. Op. Dr F. Bird.

131.—Date and age not stated. Miss ——. Incision of rather large size. (Ibid.) Not removed. Lived two years. Op. Dr F. Bird.

132.—Date and age not stated. Mrs C. Small incision to ascertain character of adhesions, and of small tumour attached to the cyst, as well as to evacuate contents. Not removed. Lived six weeks. Op. Dr F. Bird.

133.—Date and age not stated. Miss G. Incision. (Ibid.) Not removed. Lived more than two years. Op. Dr F. Bird.

134.—Date and age not stated. Mrs B. Incision; then tapped. (Ibid.) Not removed. Lived nearly three years. Op. Dr F. Bird.

135.—Date and age not stated. Miss B. Incision; then tapped. (Ibid.) Not removed. Lived six months. Op. Dr F. Bird.

136.—Date and age not stated. Miss B. Incision; then tapped. (Ibid.) Not removed. Is now living. Op. Dr F. Bird.

137.—Date and age not stated. A. B. Incision; then tapped. Tapped several times afterwards. (Ibid.) Not removed. Result not stated. Op. Dr F. Bird.

138.—Date and age not stated. Incision; then tapped. Tapped afterwards. (Ibid.) Not removed. Result unknown. Op. Dr F. Bird.

139.—Date and age not stated. Mrs S. Incision; then tapped. Tapped many times afterwards. (Ibid.) Not removed. Result unknown. Op. Dr F. Bird.

140.—Date and age not stated. Mrs C. Incision. Not tapped. Afterwards tapped and died. (Ibid.) Not removed. Recovered. Op. Dr F. Bird.

141.—Date and age not stated. Miss G. Incision to ascertain adhesions, and to remove any viscid contents. Died next day from bursting of a large hepatic abscess. (Ibid.) Not removed. Op. Dr F. Bird.

142.—Date and age not stated. Mrs C. Small incision; then tapped. Subsequently tapped, and was living more than a year afterwards. Not removed. Recovered. Op. Dr F. Bird.

143.—Date and age not stated. S. B. Incision; colloid. Was living after the lapse of ten months. (Ibid.) Not removed. Recovered. Op. Dr F. Bird.

144.—Date and age not stated. Mrs W. Small incision; then tapped. Tapped many times afterwards. (Ibid.) Not removed. Recovered from the operation. Op. Dr F. Bird.

145.—Date and age not stated. L. Small incision; then tapped. Not removed. Now living. Op. Dr F. Bird.

146.—" We have had three other operations in this town by other surgeons, all of which were fatal, and I am acquainted with another which occurred at ——, which was also fatal. None of these have been published." (The above statement was made confidentially to Dr Lee by an eminent surgeon, well known to all the Fellows of the Society.) First case here referred to, unpublished. Died.

147.—Second of these fatal cases, unpublished.

148.—Third of these fatal cases, unpublished.

149.—Fourth of these fatal cases above referred to, unpublished.

150.—Oct. 1850. "Miss B., aged 35, Yorkshire. Single female. Tumour removed with great facility, about twenty-seven pounds weight. Lived till the ninth day. Cause of death, inflammation and subsequent obstruction of bowels." (This and the following four cases have been communicated by Dr Clay to Dr Lee.) Op. Dr Clay.

151.—November 1850. Mrs P., aged 33, 9, Manchester. Tumour, ten pounds. Now well; 1851. Removed. Op. Dr Clay.

152.—November 1850. Mrs P., aged 57, Glasgow, now Manchester. Tumour, twenty-six pounds. Now quite well; —well in 1851. Removed. Op. Dr Clay.

153.—October 1850. Mrs H., aged 32. Steril. Married some years. Tumour small. Removed. Now well, 1851. Op. Dr Clay.

154.—February 1851. Mrs C., aged 45, near Lyme-Regis. Tumour removed; about twenty-five pounds. Had been tapped ten times. Was greatly exhausted previously (only fourteen days interval between tapping). Died second day.

155.—November 8, 1850. Elizabeth North, aged 23. In May 1850, enlargement of the right side of the abdomen. Tumour the size of an orange. In September it was as large as the head of a child. No fluctuation. An incision five inches in length, commencing an inch above the umbilicus. Instead of any part of the tumour appearing, several convolutions of the small intestines protruded; these, with some little delay, were returned. Anterior parietes of the abdomen adhered very considerably and firmly to the tumour on each side of the incision. Adhesions below also considerable, and appeared insurmountable. A portion of small intestine, fully two inches in length, adhered to the anterior part of the tumour. Operation abandoned. (Provincial Med. and Surg. Journal, No. 1, 1851, p. 5.) Recovered from the operation. Op. George Norman, Esq.

156.—November 14, 1850. Hannah Hiscox, aged 20. A tumour first perceived in the right iliac region in 1848. General health good. Tumour moveable. Fluctuation. Incision from the umbilicus to about an inch and a half above the pubes. Sac emptied, and root tied. Died three days after. (Prov. Med. and Surg. Journal, No. 11, 1851, p. 37.) Removed. Op. F. G. Stockwell, Esq.

157.—December 4, 1850. Eliza Smith, aged 30, unmarried. Enlargement commenced December 1849. A circumscribed ovarian tumour, extending from the left iliac region to the left hypochondriac. Fluctuating. Uterus healthy. An incision of four inches between the umbilicus, and one inch and a half of the *symphysis pubis.* Adhesive bands on both sides of the incision, separated. Incision enlarged six inches upward. Multilocular cysts. Tumour removed (weight twenty-five pounds) and pedicle tied. January 11, went to church.

Walls of the cysts composed of the following tissues—1. Of a peritonæal layer. 2. A middle or fibrous layer of unequal density. 3. A thick membranous sac containing fluid. The middle coat at the root thick, and arteries, veins, and nerves ramifying upon it. The inner coat, in some of the smaller cysts, consisted of two layers. The cysts placed one within another, like a nest of boxes. (Prov. Med. and Surg. Journal, No. 15, 1851, p. 397. Read to the Medical and Chirurgical Society, June 24, 1851.) Removed. Recovered. Op. J. Beales, Esq., Halesworth.

158.—July 1846. Mary Boyce, aged 37, mother of two children. In 1843 a tumour the size of a hen's egg, in the right iliac region, which increased till it was as large as the gravid uterus. Tapped twice. A small incision first made. No adhesions. Incision enlarged to five or six inches. Adhesions then detected between *fundus uteri* and bladder. These adhesions were considered of such a serious character as to preclude the possibility of completing the operation. (Prov. Med. and Surg. Journal, August 6, 1851.) Not removed. Died on the fourth day from peritonitis. Op. Dr Elkington.

159.—July 18, 1849. Mrs L., aged 46, five children. In 1830 first perceived a tumour in the right iliac region. Stationary five years. Two children after its appearance. In 1838, after an illness, size of tumour diminished, and remained so for six years,—it then increased. Length of incision not stated. "The tumour was connected, by extensive adhesions, to the parietes at the fore and lower part of the abdomen, and to the edge of the omentum;" it was very vascular, and supplied by several vessels, which passed from the lower edge of the omentum to the surface of the tumour. (Prov. Med. and Surg. Journal, August 6, 1851.) Removed. Died thirty-six hours after the operation. Op. Dr Elkington.

160.—April 3, 1849. Mrs Moore, aged 31, married fifteen months. Soon after began to enlarge on the left side. Supposed to be pregnant. Tumour very moveable. Irregular and nodulated, indistinct fluctuation. Tapped February 28. "The tumour was so very large that, although the incision was extended to ten or twelve inches, we were obliged to empty one or two of the cysts before we could remove the tumour." No adhesions. Recovered rapidly. April 1851, safely confined. (Ibid.) Removed. Op. Dr Elkington.

161.—Sept. 26, 1850. Mrs Howard, aged 42. Married twenty-one years. Nine children. Two years and a half since an enlargement in the left hypochondrium. Six months after a hard moveable tumour. Became pregnant, and was safely delivered. August 30, tapped. Mr Day "first drew off five gallons of ascitic fluid with the trocar, then passed a director through the opening made by the trocar, and laid open the abdominal cavity to the extent of five inches; and having ascertained that the tumour was free from adhesions, he continued the incision upwards, making use of his fingers instead of a director, to within a short distance of the ensiform cartilage. He also extended the incision below nearly to the pubis." Pedicle three inches wide, tied and divided. Recovery slow, and for some time doubtful. Diarrhœa. Sickness. Aphthous condition of the mouth and fauces. (Ibid.) Removed. Recovered. Op. Mr Day, of Walsall.

162.—May 7, 1851. Rachael U., aged 18. Enlargement of abdomen at the age of twelve years and three months, which slowly increased. Abdomen measuring forty-two inches and a half. Swelling uniform. *Os uteri* small, and high in the pelvis. "A small elastic swelling between uterus and bladder, a process of the cyst in the abdomen." Insensible from chloroform. Incision two inches below the umbilicus. Cyst attached by thready cellular connections to the abdominal walls. Punctured, and seven gallons of brownish bloody fluid discharged. Sickness. Cyst propelled between the edges of the wound, it being thought that the attachments of the cyst were of a loose and lacerable kind, the opening was enlarged upwards, and the sac separated from the abdominal walls, lumbar regions, iliac fossæ, rectum and bladder. Pedicle tied and divided. Hæmorrhage from an artery which was tied. Four or five other vessels tied. The patient pallid and faint from loss of blood, feeble efforts to set up reaction, which never came on, fell into a state of collapse, and died twenty-six hours after the operation. (Communicated by Mr Baker to the President of the Society, and through him to Dr Lee.) Removed. Op. Alfred Baker, Esq.

[To the general observations made in the article in volume sixty-first, April 1844, and those in the introductory notice, little remains to be added. Since 1844 the number of instances in which the operation has been performed, has been increased to four times the number, which had been performed at that date. The general inferences, nevertheless, to be deduced from this enlarged list of trials cannot be said to require any material alteration or any important modification. The question of the applicability of the operation in any given case before incisions are made,—the practicability of completing the operation after it has been commenced,—the mortality attending upon the operation,—and the general utility of the operation as a method of curing a disease very variable in its effects on health and the duration of life;—all these points remain very much at the present time in the same state, in which they were in the year 1844.

The first period of the history of the operation embraces a series of twenty-two years. The second embraces a space of only eight years. Yet in the latter period, in one hundred and twenty instances has this operation been attempted or performed. Such a fact says a great deal for the boldness and activity of surgeons and accoucheurs. Whether science has derived benefit or not, and whether the art of prolonging life and mitigating suffering has been improved and rendered more efficient, is a point which may be left to the judgment of intelligent readers.]

ART. V.—*Observations on Combustion of the Human Body taking place spontaneously, in reference to the Murder of the Countess of Goerlitz at Darmstadt, on the* 13*th June* 1847.

Relation Medico-Legale de l'Assassinat de la Comtesse de Goerlitz, accompagnée de Notes et Reflexions pour servir à l'histoire de la COMBUSTION HUMAINE SPONTANÉE. Par MM. AMBROISE TARDIEU, Professeur Agregé à la Faculté de Médecine de Paris, &c., et X. ROTA, D.M. de la Faculté de Paris;—Annales d' Hygiène Publique et de Médecine Legale. Tome Quarante-Quatrième. Paris, 1850, p. 191, et 363, et Tome Quarante-Cinquième. Janvier 1851, p. 99 et 383.

Long and complicated discussions, it is well known, have arisen on the question of Combustion of the Human Body, supposed to take place spontaneously. A judicial inquiry which has been conducted at the capital of the Grand

Duchy of Hesse-Darmstadt, during the years 1847, 1848, 1849, and part of 1850, and which, after exciting much interest among the German States, has extended more or less throughout European cities, has tended to awaken once more these discussions. The importance of the investigation, both in a medico-legal and in a scientific point of view, may justify a short summary of the points investigated. The full details are given in the *Annales d'Hygiène Publique et Medicale* for 1850 and 1851 ; in which also are given translations of most of the official documents which had been previously collected and published in the German language. The present notice we shall confine principally to the medico-legal part of the matter, in which the subject of spontaneous combustion has performed an important part. It may be observed, that the mistake in this case, which has so long protected the criminal J. Stauff, will be serviceable in the final elucidation of a question which has hitherto claimed probably a place too prominent in treatises on legal medicine. It is on this ground that members of the medical profession are particularly interested in the trial at Darmstadt, which doubtless would have taken a direction altogether different, had the judicial investigation been more active and more rigorous.

On Sunday, the 13th day of June 1847, about eleven o'clock in the evening, the attendants of the Countess of Goerlitz discovered the body of that lady partly consumed in her chamber, in the midst of articles of furniture which were on fire. This frightful and unexpected death remained for a considerable time involved in mystery. The idea of an accident, the supposition of an instance of spontaneous combustion, were at first put forward ; but they gave place at a subsequent period to the presumption, that a crime had been perpetrated. Long and minute inquiries were made at first, under the idea that spontaneous combustion might have been the cause of the event. The medical and chemical reports differed not a little in their evidence. The result has been that great doubt has been thrown on the whole subject of spontaneous combustion; and, as will be seen in the sequel, whatever truth may be in that doctrine, it was proved that the present case could not be referred to it, and can never at any future period be quoted as an instance of this species of death.

The apartment of the Countess, into which it was impossible to enter but by breaking the doors, and the keys of which were not found in the interior, consisted of one principal chamber with an antechamber, and a closet or cabinet ; the first lighted from the court, the second from the street, in the front of a house, the only window of which was seldom opened. At the time at which death must have taken place and the fire must have been kindled, the Countess was nearly alone in her house with the domestic John Stauff, and she could scarcely have any assistance. The prisoner was apprehended only a long time after the death of the Countess.

The Count of Goerlitz had been to the Ducal Palace to dinner, and returned home about eleven o'clock in the evening. The Countess was in the habit of locking her doors and taking the keys with her, when she remained at home, which often happened. The Countess had been seen in perfect health at three o'clock in the afternoon for the last time. She was a person aged about forty-six years at the furthest, very active, any thing but corpulent, making no abuse of alcoholic liquors, and consequently presenting none of the ordinary conditions observed and believed to exist in those persons who are the victims of the alleged spontaneous combustion. The Countess habitually enjoyed good health, though delicate; was not subject to any nervous disorder, appeared in nothing exposed to an attack of apoplexy, or to syncope, which could have caused her to fall down and to suffer an accidental burn.

Several months after the death of the Countess, namely, on the 1st of November 1847, the Count warned his domestics that a judicial inquiry would be instituted, in order to discover the cause of death; and on the following day, namely, the 2d November, mention was made of a poisoning which had taken place in the house of the Count. A male servant was accused of having taken advantage of a short absence of the cook, to throw something into a sauce intended for the Count. The greenish colour of this sauce excited the suspicions of the cook, who hastened to make known the fact. In this sauce was found a large quantity of verdigris (acetate of copper). Almost at the same time this domestic was the object of a judicial inquiry, because certain jewels which belonged to the Countess were found with the relations of the man residing at a distance from the capital.

The chamber of the Countess, it may be mentioned, is 183 inches long (=15 feet 3 inches), 166 inches (=13 feet 10 inches) broad, 160 inches (=13 feet four inches) in height. This apartment has two doors—one on the east, opening into the cabinet or closet, and another on the north, opening into the antechamber.

The cabinet, a closet smaller than the chamber, has only one window on the south side of the chamber, the window of which, directed to the west, looked upon the court of the house and its appendages. Between this window and the door of the antechamber, on the right to a person entering the apartment, and at the north-west angle of the chamber, was a secretary or scrutoire,* which was partly filled with combustible articles. At the north-east corner of the chamber is a small earthenware stove, which is lighted in the chamber. In the south angle of the chamber, obliquely in front of the library-table or scrutoire, and at the distance of twelve feet, was a sofa covered in Indian style. Above this was fixed a mirror, the glass of which was about one-fourth of an inch thick.

Opposite to the dwelling of the Count de Goerlitz stands a house occupied by a Captain Stockhausen. It was ascertained that from this house, at about eight o'clock in the evening of the 13th of June, when the daylight was still excellent, the inhabitants had beheld in the apartment a bright flame similar to that which arises from a blazing hearth, and a thick black smoke which appeared to proceed from the chimney on the north. The bright flame lasted about one quarter of an hour. Here, however, there is some discordance, in so far as it is stated in one of the pieces of evidence, that it is not known whether the smoke escaped from the chimney placed on the north or that on the south.†

The Count returned from the ducal palace at near eleven; and at this hour it is stated that there was no external sign of fire or burning. At the same time, however, when attendants forcibly broke open the doors of the apartment of the Countess, they found it filled with a thick smoke, with a suffocating empyreumatic odour; and they beheld the scrutoire of the chamber burning, though only inflamed at this instant, and communicating its flames to the curtains of the window.

Near the scrutoire, a few steps from the door, on the ground, lay the body of the Countess, considerably disfigured, and bearing traces of the action of fire. The body was directed towards the window, the legs towards the cabinet; the two lower extremities were touching each other, and were slightly inflected; the legs being bent on the thighs, and the latter upon the pelvis. The shift and the clothing were raised as far as the knee of one of the legs. The trunk was lying on the side, and the arms were slightly stretched.

As soon as the door was opened, the dead body was conveyed from the burning room upon the cushion, and thence into the sleeping apartment, exposed to the north-west of the mansion.

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* The article called *secretaire* in the *Annales*, appears to have been something between a scrutoire and a library-table.

† Annales d'Hygiène, p. 201; compared with p. 221 Evidence furnished to Dr Graff.

The writing-desk of the scrutoire, the upper and lower drawers were almost completely consumed by fire; the lower drawers more than the others. The knocker of the scrutoire, and the two lateral parts, as also some small drawers of the upper portion, were burnt only in part, so that this article had not lost its shape.

The flooring was burned before and below the scrutoire for the space of about one foot and a half, through to the joists, which were also beginning to burn. But at the place where the body was found, the flooring presented no trace that a body in a state of ignition had been placed there.

The mirror fixed above the sofa was cracked in several places, and its surface was covered with a matter the colour of which, of a deep yellow-red, resembled, according to the deposition of an eminent painter, M. Lucas, that which is extracted from the skull of mummies, and which is known by the name of Mummy. The glass was so covered with this coating, that it was not visible except at the parts where this matter had been scraped off while it was still soft. This matter became dry, and adhered to the glass, on which it was deposited after the atmosphere of the chamber was charged with it. In this coating were seen by a lens a quantity of minute black points, which were visible to the naked eye only when the glass was viewed horizontally. Upon the covering of the sofa in the chamber, and upon the chair nearest to the scrutoire were found traces of burning. The feet of the chair were slightly charred.

The bell-rope was found on the ground, burned at its superior extremity below the place where it had been fixed.

In the closet was a sofa, in the middle part of which was a hole about one inch and a half large, produced by the fire, which had destroyed the cloth, the hair, and the other stuffing sea-grass (*zostera*), as far as the girths. This fire was not discovered and extinguished until midnight, one hour after the parlour had been entered.

In the cabinet, where the burning sofa was found, an oil-painting placed above it was so covered with the same deposit, that the colour of the painting had disappeared.

Near the same sofa was found the shoe of the Countess, which had been missing when the body was conveyed into the adjoining apartment. The candlesticks, furnished with tapers, were in their place; and no circumstance indicated that she had used them, and procured light.

The body of the deceased Countess, when examined in the chamber to which it had been conveyed, was in the following condition.

The head was all over uniformly burned, was reduced to the volume of two fists, and could no longer be recognised but as a brown, charred mass. The remains presented a deep brown colour, with a certain degree of lustre like a coating of varnish.

The neck, like the head, was burned all round, had the same aspect, but appeared to have lost less of its bulk than the head.

The marks of burning extended as far as the back of the Countess could be viewed without turning the body.

On the anterior part of the body, the marks of burning were prolonged over the chest almost to the pit of the stomach. From this point they were extended, in the form of an arch, on both sides of the chest, ascending in such a manner that the articles of clothing which were untouched, were found almost at the same elevation as those parts of the body which had not been burned.

At the lower part of the chest, at the point at which the marks of burning were limited by the skin, the latter made at the level of the burned part a prominence of about one inch, and was slightly charred towards this margin. Above this point, and anterior to it, the dress, and the parts of the person, excepting the sternum, the collar-bones, the ribs, and the intercostal muscles, were so uniformly burned, that the observer might have concluded that the skin of the breasts and the muscles of the chest had been removed by the knife. Over this burned surface, the fleshy parts, and especially the intercostal muscles, had a deep brown colour; but they had less of the peculiar lustre than the head, and it was possible to distinguish the bony parts of this region by the dark gray aspect which they presented.

The two arms appeared, in the part where the position of the body allowed to be observed, to be charred in a uniform manner from the tips of the fingers to the shoulder, but the textures were still capable of being distinguished. They were only more deep than the other burned parts, and they were entirely black.

All the fingers were forcibly inflected. The two hands were so in a slighter degree. The right fore-arm was inflected upon the arm, so as to form with the latter a right angle. On the left side the degree of inflexion was less marked.

The head of the left humerus, which had perforated the capsular ligament and the deltoid muscle, formed a projection of about three inches directly upwards. Dr Siebold did not find the soft parts charred upon the prominent portion of this bone, but beneath the shoulder, the soft parts covering the left fore-arm and left humerus were charred. It may here be added, that one of the persons who assisted in moving the body stated in his declaration, that having taken the dead body under the arms in order to raise it, it appeared to him that at this moment the bone of the arm (*humerus*) came out of the shoulder, and that in his hand was left a soft fatty mass.

The two arms, which were still attached to the thorax, were only little separated from it; the right a little forward, the left directly backward.

The fore-arms though more separated, were only at one foot from this part of the body, which was covered by articles of dress which were not burned.

In no part were there redness or true phlytrenæ. Witness stated that they had seen these objects; but their existence has not been established by physicians. The reporters remarked upon the body no lesion which could indicate marks of violence preceding the marks of burning.

The account now given, contains all the material information which was collected immediately after death, as to the state of the dead body. No anatomical examination, strictly so called, appears at that time to have been made, so as to ascertain the state of the internal organs and external parts, further than what was ascertained by inspection of the body.

On considering the question of the cause of death, Dr Graff, physician to the Count, gave, on the 14th of June, the day after the occurrence, a report in which he stated that, from the circumstances enumerated, namely, that the Countess was occupied at her secretary, that she had been unable to remove from it or call for aid, and that the marks of burning were in the greatest degree at the head, it appeared very probable that the deceased had been the victim of spontaneous combustion. In a second report, however, on the 15th of June, he allowed that spontaneous combustion ought to be admitted only in the circumstances that no violence could have been exercised on the victim. He added that this was possible; and that the burnings on the head and neck might have been means employed to disguise the marks of violence. If, he added, we suppose strangulation to have been first produced, this hypothesis is supported by the fact of the protrusion of the tongue from the half-opened mouth. It will be seen afterwards how important was this inference.

The medico-legal mind, and probably the judicial spirit, of Darmstadt was very considerably perplexed. Nothing very active appears to have taken place till the 1st of November, 1847, fully four months after the event, when the Count de Goerlitz, as already stated, announced to his servants, that a judicial inquiry was to be undertaken. Meanwhile, the idea of spontaneous combustion had taken possession of several minds, and especially of that of Dr Siebold, who became and continued for several months the strenuous advocate of this hypothesis. His proofs and arguments are contained in a report dated the 12th of April 1848.

The report by Dr Siebold is one of considerable learning and ingenuity. After stating the principal facts relating to the manner and the circumstances in which the body of the deceased lady was found, he draws the conclusion that it is impossible to explain the degree and the marks of burning upon the body, either by ordinary combustible materials or by that of alcohol. "From the particular mode and the character of the burning of the Countess, he says, it is beyond doubt, that the burning has not been produced by the sole, and perhaps accidental combustion of the library-table, and so small a part of the flooring."—"Supposing that the fire had been lighted with wood, with charcoal, or pit coal, its smoke, as well as the repulsive odour produced by the combustion of the dead body, and with which the chamber must have been very speedily filled, rendered it impossible for any one to remain there for several hours." He explained the different marks of burning in the furniture, and the flame and smoke actually seen when the chamber was broken open, by suggesting that they must have spread from the burning body of the Countess to the nearest articles.

In forming this inference, he is met at first by the difficulty, that, in most cases of spontaneous combustion, the persons are addicted to intoxication. The Countess of Goerlitz was so far from this, that she was exceedingly temperate. Dr Siebold, therefore, attaches little importance to this habit as an antecedent or predisponent to spontaneous combustion. It would not be difficult to shew that the habit is a proof of no value; and that it is quite as likely to be antecedent of common combustion, as of what is called spontaneous combustion.

Assuming, then, that fire and flame had burst out spontaneously about the head and neck of the departed Countess, and that this had extended to her dress, he argues that it explains the following facts.

1. The origin of the hole burned upon the ottoman, and corresponding, on one hand, to the spot on which the Countess may have rested her head, and on the other to the size of her head.

2. How the inhabitants of the opposite mansion beheld in the direction of this burned hole a very brilliant flame.

3. The appearance of the flame at eight o'clock in the evening, proves that the spontaneous combustion had commenced at this hour; that the Countess had been the first and principal victim of the conflagration; that the fire had afterwards extended to the ottoman and secretary. All these are proved by the prints of black smoke which are found on the surface of the mirror.

The first person who called in question this hypothesis was Dr Stegmayer, who, in a report, dated 27th December 1847, expressed the opinion, that it was a question to which it was impossible to give an answer either affirmatively or negatively. After a long interval, a report appeared in the name of the Medical College of the Grand Duchy of Hesse by Dr Graff, dated the 14th July 1848, giving an elaborate view of the question. In this report, the College pronounced a series of twenty tests or characters by which cases of spontaneous combustion were distinguished, and shewed by comparative examination, that to few or none of these could the case of the Countess of Goerlitz be applied. It is unnecessary to recapitulate these tests, because after all more or less doubt rests upon several of them.

The test to which Dr Graff, who is the author of the report, attaches most importance is that contained in the eleventh proposition, namely, That spontaneous combustion is attended with flame, and can with difficulty be extinguished by water. If this be considered, he argues, it is impossible to doubt that in the present case there was no spontaneous combustion; for this sole reason, that the most essential conditions to such an event were totally wanting. Dr Graff finally adduces the following experimental fact.

A dead body was lying on the table of a dead-house, in which opened the door of the stove of the theatre of anatomy. The part of the table on which rested the head of the body touched almost the door of the stove; and beneath this table were some wood shavings and splinters. About noon the fire of the stove was kindled. Very soon was felt a smell of smoke; and when the attendants came to search for the cause, they found the shavings and the upper part of the table burned, as well as the head of the dead body, to so great a degree, that in its place was left only a shapeless and charred mass of repulsive appearance. The fire had been communicated to the shavings by the door of the stove being left open, and these had set the table on fire. In this manner was burned the head of this dead body, without any other combustible material than a small quantity of shavings and the top of a wooden table.

It may be remembered that Dr Graff at first threw out the idea of spontaneous combustion to account for the appearances on the body of the Countess of Goerlitz. This, however, which was manifestly a hasty opinion, he speedily modified. After the interval of more than twelve months, upon more deliberate reflection and more careful study of the facts, he maintained that it may with certainty be admitted, that the occurrence of spontaneous combustion was not in the circumstances possible, and that the death of the Countess of Goerlitz must positively have been the work of an assassin.

As the latter inference was one which required, if possible, more decisive and positive proofs, the body of the Countess was disinterred on the 11th of August 1848, and subjected to anatomical examination in the presence of Drs Graff, Buchner, Rieger, and the Surgeon Freniard.

The essential facts brought to light by this proceeding are the following:—

1. The black colour, and the complete or incomplete combustion of the most of the bones of the skull.

2. The charred state of the bones, principally at the level of the sutures on the left side.

3. The total destruction of more than two thirds of the bones of the skull, on the superior region and the left side of the head.

4. The rather firm consistence, the smooth aspect, and grayish-white colour presented by the remains of the osseous case, when it was cleared from the black coating by which it was covered.

5. The clear normal sound emitted by the bones.

6. The separation of the two jaw-bones.

7. The brown-grayish colouration of the vertebræ, the collar-bones, and the ribs, the tissue of which was close and very dense.

8. The delimitation of the burning at the pit of the stomach.

9. The destruction of the organs of the chest, with the exception of some remains of the heart and the diaphragm.

10. The charring of the head of the left humerus, a part of which was completely consumed.

11. The hardness and the solidity of all the bones of the arm, which emitted a normal sound upon percussion.

12. The state of the *parietes* of the abdomen and of the thighs, which had the consistence of leather and the aspect

of dry cheese, and in which putrefaction had made but very little progress.

13. A fissure seated on the right temporal bone, almost perpendicular to its superior margin, and nearly half an inch long. This fissure Dr Graff observed only on the 12th (that is, the day after the inspection), while examining the rest of the skull at the window, and with oblique light.

From the whole of the facts now enumerated, the following conclusions are deduced:—

1. The flame which produced the burning of the Countess of Goerlitz had acted principally upon the left side of the body.

2. The flame was directed especially towards the head.

3. The burnings were not produced by spontaneous combustion.

This inference, by far the most important, is corroborated by the following arguments.

a. The body differed in no respect from a body which had been burned in the ordinary manner, and which had remained interred for fourteen months in a dry sandy soil.

b. Spontaneous combustion produces a sudden destruction of the body, which is reduced to ashes with the exception of some remains of the extremities and of the head. In this case there was no incineration.

c. Spontaneous combustion predisposes to a rapid decomposition of the fluids as well as of the solids, and thereby induces rapid putrefaction of the parts which have escaped the action of the fire. In the instance of the Darmstadt case, the soft parts actually burned were the only putrefied parts. Those which the flame had left untouched externally were dry and tanned internally and little affected by putrefactive decomposition.

Other proofs are derived from the soundness and resistance of the bones. Even the head of the humerus which had been charred, was firm and resisted the action of steel instruments. All these characters the reporters argue, conformably to those presented by an ordinary dead body which has been interred for one year, furnish the most direct proofs that there was no spontaneous combustion in the case.

4. The separation of two inches which was observed between the superior and inferior jaws, accords with the observation concerning the open state of the mouth, which is recorded in the report of the inspection of the body, and it has no other value than to confirm this fact.

5. The decomposition of the thoracic viscera, in opposition to the scarcely beginning decomposition of the abdominal

viscera, depends on the circumstance that the effects of the burning had penetrated into the interior of the chest.

6. The fissure, which was found on the right temporal bone, may have been produced by means of fire; but it is also possible that it may have been the result of external violence inflicted upon the skull by mechanical force.

The opinion of the College definitively therefore was, that the death of the Countess was not to be ascribed to spontaneous combustion; that the body had been subjected to the action of fire, probably after death; and that the burning had probably been means employed to conceal a murder previously effected by strangulation or by blows upon the skull. This opinion was presented to the legal authorities on the 1st September 1848.

Dr Siebold still adhered to his opinion; and he published a second memoir, reproducing and enforcing the arguments already adduced. To this again Dr Graff opposed a second refutation. It is scarcely necessary to say that these two performances throw no material light on the matter at issue, nor tend in any way to dispel the doubts hitherto entertained.

The President of the Assizes of Darmstadt considering that the contradictions which prevailed in the previous reports rendered necessary new information, issued, on the dates of February 18, and March 23, 1850, two separate decrees directing that M. T. Bischoff, Professor of Physiology at Giessen, and Justus de Liebig, Professor of Chemistry in the same University, should form a commission, joined with the Medical College of Darmstadt and other scientific assistants already named, to inquire into and consider the circumstances relating to the death of the Countess of Goerlitz, and to give as direct and satisfactory answers as possible to the following questions.

I. From the circumstances specified, is it possible, probable, or certain, that the Countess of Goerlitz was destroyed by an attack of what is termed spontaneous combustion, the consequence of which brought about the condition in which she was found on the 13th day of June at 11 o'clock at night?

To this question, all the reporters are unanimous in answering, No.

Dr Graff admits the possibility of the occurrence of spontaneous combustion in general, but thinks it not probable in this particular case. The majority of those consulted, namely, Messrs Rieger, Buchner, Leydecker, Merck, Hohenschild, Liebig, and Bischoff, reject spontaneous inflammation and combustion as impossible; and consequently consider it as inadmissible in the present case. This decisive course was produced by the reasonings of Messrs Liebig and Bischoff; and to these reasonings, not only did Drs Buchner and Leydecker, but eventually Von Siebold profess themselves converts.

II. In the circumstances specified, is it possible, probable, or certain, that the Countess of Goerlitz was destroyed by the effects of fire taking place externally, and that she had been exposed to these effects either by an unfortunate accident or by the design of her own act, or by the act of another person?

To this question, all the reporters equally answered, No.

III. In the circumstances specified, is it possible, probable, or certain, that the body of the Countess had been exposed to the action of fire after death; and in this case, are we to admit that life was lost by suicide or by the hand of another person, by means of fracture of the skull, or strangulation, or by a morbid state, or by an unfortunate accident?

To the first part of the question, all answered unanimously, Yes.

As to the question of suicide, scarcely any circumstance favours such an inference. Suicide by means of fire they believe impossible.

As to suicide by means of poison, had such means been employed, the Countess must have been able first to have set fire to and burn the sofa, the ottoman, the back of the mirror, and even the writing-table; and then placing herself before the object to have taken the poison. This however is contradicted by almost all the circumstances in which the body was found as well as by common probability. The character and habits of the deceased were further altogether at variance with such a supposition.

As to the kind of poison suggested, the persons consulted say that it would no longer be possible to make chemical researches for the poison. The time which has elapsed would only permit to search for a metallic poison; but the rapidity with which death took place, and the absence of all external traces, permit not to admit that this kind of poisoning took place.

This is controverted by the author in the *Annales d'Hygiène*, who remarks, that numerous facts prove that, after a period of interment still more prolonged than that of the Countess (at this time three years), it was possible to recover, by analysing the remains of the dead body, traces of the poison, the presence of which the state of the organs could not make to be suspected.

With deference to the author, we must be allowed to observe, that he mistakes the meaning of the reporters. What they say is, not that a metallic poison cannot be detected after such a time, which all good chemists know to be easily done, but merely that the rapid way in which death took place, that is, within seven hours, probably within five, is inconsistent with the supposition of any metallic poison having been used.

The presence of hydrocyanic acid, they continue to say, it would be impossible to establish after the interval of three years. The detection of morphia, strychnine, or any similar poison, would be most problematical.

The reporters think it improbable that the Countess was cut off by any morbid state, or any accident; and equally unlikely that the body could have caught fire during such morbid state, or after such accident.

The principal morbid states which could have paved the way for such an accident, are apoplexy, epileptic seizure, or a fainting fit, such as syncope; and to neither of these disorders was the Countess subject; nor was it known that she had ever evinced any symptom leading to the suspicion of such attack.

In this manner we come, not only by the method of exclusion, but further, by direct reasons, to consider, as a highly probable event, the murder of the Countess of Goerlitz. It may be matter of doubt, that the burning was the act of a third person. The protrusion of the tongue is not a sufficient proof of strangulation, when we reflect on the degree of burning of the soft and bony portions of the face, and on the different action which fire exercises on uncovered parts, and upon those which are more or less deeply concealed.

As to the fissure observed, after disinterment, upon the right temporal bone, and which, according to the Medical College, was perhaps the initial point of a fracture extending to the base of the skull, it may quite as well be the effect of the action of fire, as is shewn by the presence of similar fissures upon a skull which was burned experimentally in the course of experiments made at Giessen. Further, numerous fissures identical to the first were produced, by the mere act of touching, upon other parts of the skull. It is therefore not surprising that one of these fissures should have been observed on the first day of disinterment, while others appeared gradually at subsequent periods.

It is, however, more probable that the Countess was knocked down than strangled. It is not probable that a man of ordinary strength was able to seize and stifle, without active resistance, a robust woman, accustomed to corporeal exercise, as was the Countess, unless she had been surprised in a state of sleep, which can scarcely be supposed.

IV. In the circumstances enumerated, is it possible, probable, or certain, that the combustion of the body of the

Countess was caused solely by the burning of the secretary, or had the fire, of necessity, another origin?

Upon this question the consulted persons have not been agreed.

The majority think it possible and likely that the combustion of the body of the Countess may have been produced by the sole present and visible cause of the burning, that is, by the burning secretary. The temperature, and the amount of heat developed by this source, were sufficient to effect the burning of the body of the Countess, the upper part of which was about two feet from the burning piece of furniture.

In proof of this inference, the reporters give the following estimate.

Calculations made separately by two separate parties, to shew that there were burned at least seven cubic feet of timber, the greater part of which was oak, the smaller part fir and mahogany. These seven cubic feet are equivalent to one hundred and twenty-five pounds of timber, estimating at least eighteen pounds to the square foot.* It is proved that this wood gave a residue of two cubic feet of charcoal, which, estimated at seven pounds each cubic foot, are equivalent to sixty pounds of wood. Consequently, sixty-six pounds of wood had been burned.

The mean weight of the body of a female, aged from forty to fifty years, is one hundred and thirteen pounds. Admitting that the fourth of this weight had been dried and reduced to charcoal, which is higher than the reality, the heat developed is not above seven or eight in the hundred for the materials supposed to be consumed. The rest of the increased heat would, they argue, be employed in raising the temperature of the apartments.

They remark, that on the library-table were burnt several objects, of which no account could be obtained. The heat, they add, was sufficient to cause the fusion of various metals —gold, silver, iron. Most of these metals are only melted at the temperature of one thousand degrees, while from three to four hundred degrees are sufficient to char animal substances, and to consume them completely when their action is continued. Stearine candles were melted at a distance of from nine to twenty-seven feet, a fact which supposes a temperature of at least fifty-eight degrees. Several chairs, more distant from the centre of the burning than the dead body, took fire.

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* This is the statement in the *Annales*; but it is evident that *cubic foot* is meant.

The time during which the burning of the library-table had been capable of acting on the dead body, was sufficient to produce the effects mentioned.

It appears to the reporters certain, that the burning of the library-table had commenced early in the evening, after the departure of the Count, towards about half-past seven. From this moment till eleven o'clock there are between three hours and three hours and a half, which were quite sufficient to produce the effects observed.

The position of the dead body favoured its burning. It was only about two feet from what must have been the centre of the fire.

The form of the burning perfectly corresponds to this hypothesis, that it would have taken place at the spot and in the position in which the dead body was found. The head, neck, and upper parts of the body were burned to charring, while the lower half of the body and the lower extremities had escaped. These phenomena, namely, the parts of the body burned, while others were not burned, give a more correct idea of the position of the victim than the imperfect depositions and contradictory testimonies of witnesses who were frightened and shocked, or whose observation was disturbed by smoke, smell, and darkness.

An experiment made at Giessen, with the express purpose of reproducing the circumstances of the Darmstadt case, and which was made by exposing a human head to burning materials, shews that the combustion might have been produced by the library-table, and in the position in which the body of the deceased Countess was found, especially if it be remembered that this article contained paper, boxes, and similar objects.

The reasons now adduced come under the head of what may be called positive arguments.

Some other reasons of a negative character there are, which concur to shew that it seems unlikely or impossible that a more considerable fire than has been now supposed, had been employed by any third person.

1. No positive facts shewed that the body of the Countess had been burned at any other part than in the chamber, except the smoke issuing from the chimney of the domestic.

2. Neither can it be admitted that the combustion had been effected in the apartment by means of alcohol, oil, charcoal, or wood. The manipulations necessary to an operation of this kind, the business of maintaining the fire, the changes in position necessary to be given the dead body, the employment of supports for the head and chest, and of peculiar dis-

position in order to burn the arms in the manner in which they had been burned, all this appears impracticable to be performed by a single person. In order to fulfil each of the conditions now mentioned, there must have been required preparations, which would have left traces on the flooring, on the tables, on the chairs, the vessels, and other objects, all of which the most minute searches failed to discover.

3. The form of the burned parts of the body, the head, arms, chest, and even of the clothing, cannot be explained by supposing successive changes in the position of the body.

4. It seems impossible that any person could have been able to remain shut up in the chamber in which the body and articles of clothing were burning. The burning of the garments and hair alone, which are first caught in fire, must have filled the room with so great a volume of smoke, that no person could long remain there. Nor is it probable that the doors or the windows had been opened. Had the first been open, they would have made known the burning in the interior of the house. Had the windows been open, the conflagration must have been made known externally. It may be added, that the fire would also have been greatly increased by the admission of the air, and consequently must have been discovered long before the time at which the doors of the apartments were broken open. Still less possible is it to suppose, that any person could have remained in the chamber, surrounded by the products of combustion.

5. The body of the Countess was not burned upon the library-table. To this end only the table drawer could have been employed, because it is proved by witnesses that the two upper drawers were locked. But had this been the case, the fire would have attacked the drawers as it did below the table itself.

6. Alcohol could not have been employed in burning the dead body ; because, in the first place, a large quantity of this liquid would have been required, and the transport and combustion by this, must have presented difficulties, of which no trace is found. In order to burn the head alone, three litres of alcohol would have been required. To burn the chest and arms, a still greater quantity was needed. In the second place, a large reservoir would have been necessary, in order to expose in it the dead body to the flames thus produced. But even if it be admitted that three litres were sufficient, the burning of this with the body, must have so vitiated the air, that no person could have remained in the chamber.

7. The burning of the ottoman in the manner, and to the degree in which it was burned, excited much attention. This

article, the reporters think, may have been burned either by accident, or to keep up the fire, or to destroy the traces of blood. Supposing the fire to have broken out in the evening, they attach little importance to the first supposition. If the fire had been communicated accidentally from the library-table to the ottoman, the time necessary to the development of the conflagration, must have been insufficient to allow it to be seen by the inhabitants of the opposite house, who, at 15 minutes past eight in the evening, saw a fire in the direction of the ottoman. The other two suppositions would imply that some living person was in the apartment during the time of the fire,—a supposition which has been already shewn to be highly improbable. Further, the depositions of the witnesses who beheld this flame, lead to prove that no person was seen near this fire.

The minority of the commission, without dissenting altogether from the above stated views, presented the following modification of them :—

The burning of the library-table they allow was perfectly capable of communicating fire to the body of the Countess and causing the combustion of the body, even in a degree more advanced. But the circumstances of the event induce them to believe, that the burning of the body was not exclusively effected by the burning of the library-table. These contradictory circumstances are the following :—

1. The peculiarity of the distinctive effects of the burning in themselves.
2. The absence of all trace of burning at the spot at which the body was found.
3. The fire of the ottoman, which for the space of fifteen minutes was beheld from the opposite house.

The most important remark made in illustration of these considerations is this ; that, whereas the inhabitants of the opposite house beheld a blazing flame, the materials of which the ottoman were made, namely, a two-inch thick mattress of hair, burn with great difficulty, and when set on fire in several places, they cease to burn at the end of two minutes. Upon this subject an upholsterer at Darmstadt had performed some experiments, which shewed that horse-hair burns with great difficulty, and without producing an elevated flame. The reporters therefore infer, that this blazing flame had not been maintained by the hair of the cushion only.

Such were the reasonings and considerations which the College of Darmstadt, with their scientific assessors submitted to the court, regarding this apparently mysterious transaction. Though there is still some vacillation and contradiction, yet it is manifest that the reasonings on the facts, pointed more directly than heretofore to the conclusion, that not only was spontaneous combustion altogether out of the question, but that in all human probability the Countess had been put to death, and the body had then been burned, in order to remove or render impossible to be known, the means by which life had been violently terminated.

Another scene in this drama was yet to be presented. Spontaneous Combustion had made so great a figure in this inquiry, and had been so long the means of creating a great delusion, that it seemed to be desirable to inquire into the grounds upon which this doctrine had been admitted among the causes of the sudden and unlooked-for extinction of life. This has been done by Professors Bischoff and Liebig. An abstract of the views of these philosophers is all that can be given.

The first instance of alleged Spontaneous Combustion which is recorded, is referred to a time about one hundred and fifty years ago. This instance was afterwards followed by others. Upon examining what is understood by Spontaneous Combustion, it is supposed, that a person may have been burned, that his body may have undergone the action of fire, without having been placed in those external conditions in which can be found an adequate explanation of the phenomena; and even when combustible materials are found, they appear not capable of maintaining combustion. It is then said, that the person so found with marks of burning has not been burned by external materials; that he has been burned of himself or spontaneously. This expression, which is quite hypothetical, appears to be unwarranted. It ought to have been said—we know not in what manner this man has been burned. By saying that he has been burned spontaneously, for a fact imperfectly known, an explanation equally obscure is substituted. This doctrine of spontaneous combustion therefore, which has crept into science, is not the result of knowledge or of experience, but of ignorance.

Let us consider upon what authorities this pretended observation is founded. Among the cases which have been published, and which are in number between forty-five and forty-eight, the inquiry relates to persons who have been burned either in the presence of an insufficient quantity of combustible materials, or without any appearance of combustible materials. With the exception, however, of two cases of alleged complete spontaneous combustion, and four instances of partial burning, where the sufferers have survived, in no instance has any person with scientific authority, in none even has a single witness been present at the accident. The first instance of alleged complete spontaneous combustion, was observed and related by a chamber-maid, and an unknown person; the second by an unknown person.

One of the earliest cases is the following.

At Rheims in 1724 and 1725, lived a man named Millet, whose wife was daily in a state of intoxication. The housekeeping was conducted by a very pretty young woman, a fact which must be kept in mind, in order to form an estimate of the circumstances now to be mentioned.

The wife of M. Millet was found burned dead on the 20th of April 1725, in her kitchen, one foot and a half from the hearth. Part of the head only, a portion of the lower extremities, and some vertebræ, had escaped destruction. One foot and a half of the floor under the dead body had been destroyed by fire. A kneading-trough and a salt-tub, very near the burned parts, had received no injury. M. Chretien, a surgeon, removed the remains of the dead body with all judicial formality. John Millet, the husband, declared that on the 19th, the previous night, towards eight in the evening, he had gone to bed with his wife; that she not being able to sleep, had gone into the kitchen, where he thought she was warming herself; that he, Millet, had fallen asleep, and was awakened about two in the morning by a strong smell; that having run into the kitchen, he found the remains of the body of his wife in the state described in the judicial report.

The judges pursued this case with activity. The good-looking servant girl was in some measure the exciting cause of the ruin of Millet, who bore all the rigour of the law, under the suspicion of having murdered his wife and then burned the body. He appealed to the supreme court, where the case was considered as one of spontaneous combustion. He was finally acquitted. But his spirit was broken, his character was ruined, and he was reduced to such poverty that he was obliged to spend the rest of his days in an hospital.

That this was a case of spontaneous combustion, there is no proof beyond the assumption. The woman might have fallen asleep near the fire, and thus have been burned to death.

The oft-repeated story of the Countess Cornelia Bandi, of the town of Cesena, 4th April 1731, is a very silly and fabulous relation, abounding in those marvellous circumstances which weak minds take pleasure in remarking. In this case were noticed the black soot, supposed to be something peculiar, and the offensive odour believed to distinguish combustion called spontaneous. It is totally forgotten, that whenever animal matter is burned to charring, the soot is always greasy, and the air is loaded with a peculiar fetid smoke.*

The case was manifestly one of ordinary burning by combustible materials.

The instance of the woman Grace Pett, the wife of a fisherman at Ipswich, which took place on the 9th of April 1744, and which is recorded in the forty-third volume of the Philosophical Transactions, is far from being like a case in which the body had taken fire in any unusual way; it seems quite like an instance of a drunken woman with her pipe, getting into the half-drowsy somnolent state in which these persons often are, and allowing the ashes of the pipe to set her clothes on fire. This woman had on a cotton gown.†

The instance of Mary Clues, recorded by Benjamin Wilmer in the sixty-fourth volume of the Philosophical Transactions for 1774, as having occurred at Coventry in March 1772, is altogether similar. A woman of fifty-two years, a habitual drunkard, who seldom passed a day without drinking from half a pint to a quart of rum or anise-seed spirit, and who often drank in one day four half-pints of rum, falls into a state of bad health with jaundice and debility so considerable, as to oblige her to keep her bed. She continues in this state, her old custom of drinking, and smoking a pipe of tobacco. The bed of the chamber in which she lay was parallel to the chimney or fire-place, and about three feet distant. On Sunday morning, the 1st of March, she fell on the ground, and, from her weakness, was unable to rise; and in this position she remained till a neighbour entered the apartment and placed her in bed. The following night she remained alone, by her own wish: an attendant having taken leave of her at half-past eleven, and, according to custom, locked the door. This person had placed two large pieces of coal on the fire, and set on a chair at the head of the bed a lighted candle in a candlestick.

At half-past five in the morning, smoke was observed to issue from the window. The door was quickly broken open, and some flames which were blazing in the room were easily extinguished. Between the bed and fire-place were seen the remains of the body of Mary Clues. The legs and one thigh were still entire; but of skin, muscles, and viscera nothing remained. The bones of the skull, of the chest, of the spine, and of the upper extremities, were entirely calcined and covered with a white efflorescence.

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* Philosophical Transactions, vol. xliii. for 1745, p. 447.

† *Ibid.*, p. 464.

The side of the bed which looked towards the fire-place had suffered most; the wood of it was superficially burned. The feather-bed, the bed-clothes, and the coverings were not. The reporter adds, that two hours after the door was opened, he observed that all the walls and all the articles in the place had been blackened, that an unpleasant vapour filled the place, but nothing but the dead body presented strong impression of fire.*

It is not easy to see why this instance should have been removed from the ordinary class of examples of drunken and helpless persons setting themselves on fire. Either the candle had set her clothes on fire, or Mary Clues had come out of bed and set herself on fire, and was then, from her weakness and helplessness, unable to cry for aid, or make any efforts to extinguish the flames.

If other cases, such as that of Madame Boiseon, given by M. Boinneau, curate of Plerguer par Dob, to Lecat on the 22d February 1749;† the case of Marie Jauffret, in February 1779, recorded by Muraire;‡ and that of Madame Thouar, in June 1782,§ communicated by Merille,—be carefully examined, all will be allowed to present the same circumstances, and the same antecedents. It may be added, that in this last case, which is described as that of Mademoiselle Thouar, the lady was aged above sixty years. She was very corpulent, greatly addicted to the use of wine and spiritous liquors. On the day of her death, she had drunk three bottles of wine and about half a quart of strong spirit.

The first case of alleged partial combustion is that of the priest Bertholi, recorded in 1786 by Joseph Battaglia, a surgeon at Porto-Bosio. This ecclesiastic had been to the fair or market of Filetto on business. He was to spend the night with one of his brothers-in-law. He had come home and retired to his chamber in a state of perspiration. On lying down in bed, he placed a napkin or towel between the shoulders and the shirt, and then began to read his Breviary. Some minutes after, an extraordinary noise and screams were heard; the people in the house ran to the chamber, and found Bertholi stretched upon the pavement, and surrounded, it is said, by a light flame, which receded as they approached, and finally disappeared. A lamp previously filled with oil was dry, and the wick was reduced to ashes.

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* Philosophical Transactions, vol. lxiv., p. 340, 1774. And Cases and Remarks in Surgery, by B. Wilmer, Surgeon. London, 1779. P. 255.

† Lecat. ‡ Journal de Médecine, tome lix., p. 440.

§ Journal de Médecine, tome lix., p. 140.

When Bertholi was placed in bed, the physician who visited him found the integuments of the right arm almost entirely detached from the muscles, and hanging in shreds, and the skin of the fore-arm in the same state. The integuments of the sides were greatly injured. These shreds were removed, and the right hand was sacrificed. The shirt as well as the cowl of the patient was reduced to ashes; yet the hair of the head and the napkin placed between the shoulders were uninjured.

The patient, at the moment of the accident, felt as if struck by the blow of a club upon the right arm, and beheld as if a blue flame were attacking the shirt and reducing it in an instant to ashes.

The day after the accident, the whole right arm was in a state of complete gangrene. The second day after, gangrene had taken possession of all the burned parts. The patient was distressed by constant vomitings, burning thirst, violent convulsions, voiding putrid offensive stools; he was delirious and feverish. He then fell into a state of coma, while putrefaction was making progress so as to cause the person to exhale an offensive odour. This state lasted two days; and death took place on the fourth day from the evening of the occurrence.

"Whoever reads this case without prepossession," says Professor Bischoff, "will be convinced that this man was burned only because his clothes had caught fire at a lamp." "To this fact," says Professor Liebig, "which forms the principal argument for the possibility of spontaneous combustion, is attached the idea of a fire of a peculiar nature, which consumes the animal substance without inflaming surrounding combustible substances."

"If we consider," he continues, "that the body was burned only at the spots at which the shirt was applied; that everywhere else there was no trace of fire; that the skin was neither burned nor charred, but only detached from the body, hanging in shreds; while the shirt was burnt and reduced to ashes, it is impossible to admit that the fire was communicated to the shirt by the skin, which itself was not burned. No other explanation is then possible than this,—which admits that the shirt had first caught fire, and that the burnings of the body were the consequence of this superficial scorching. The presence of the lamp, the total consumption of the oil, and the wick reduced to ashes, remove all doubts as to the origin of the fire."

The case of Bertholi has for a long time been the stronghold of the hypothesis of Spontaneous Combustion. When examined carefully and rationally, it vanishes into nothing. It is merely the common history of a person reading in bed, and in the half sleep which often overtakes a person in such circumstances, allowing his clothes to catch fire.

A second case of partial alleged spontaneous combustion relates to a blacksmith named Pierre Reynateau. This man, who was aged forty years, said to be sober, and of good constitution, resided at the village of Léegnon, two leagues from Bourdeaux. On the 7th of September 1822, at four in the afternoon he was returning from Bourdeaux, whither he had been, to Léegnon, when about a quarter of a league from his own house, he felt himself struck by a violent blow on the thigh. He saw no person who could have given the blow. But when he placed his hand on the seat of the blow, the index finger was covered by a blue flame. The middle finger then took fire. The flames were quenched by water.*

The narrative of this case is corroborated by no serious evidence; it is filled with contradictory statements; and it is terminated by this characteristic circumstance, that nothing but consecrated water could extinguish the fire. Such a story merits no credit; and it must be useless to examine its circumstances.

The third is the instance which is represented to have happened to Margaret Frederika Catherine Heins, a girl aged seventeen years, residing at Hamburg. This girl, a sempstress, was small, delicate, and though externally in flourishing health, was nervous and hysterical.

This girl, when engaged in sewing on the evening of the 21st of January 1825, all of a sudden felt a sensation of unwonted heat in the whole body, and a feeling of violent burning in the index finger of the left hand at the moment when she wished to take a bit of wax. At the same instant she beheld her finger surrounded with a blue flame to the extent of one inch or one inch and a half, and which emitted a peculiar sulphureous odour. Against this flame, affusion of water and a wet napkin in which the finger was wrapped were of no avail. The fingers were repeatedly immersed in water, and the whole hand seemed then to be on fire. The patient hastened home with her hand in her handkerchief. The fire communicated to her clothing, but the flame was visible only in the dark. At home she had recourse to milk, several applications of which she made during the night until she succeeded in extinguishing the flame. A sulphureous odour nevertheless remained, and was renewed from time to time. A bloodletting and the use of medicine were followed by relief. But the left fore-arm was the seat of a burning heat, and exhaled at times a sulphureous odour.

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* Gazetta di Milano, 7th Avrill 1823.

On the 5th of February this young woman was received into the General Hospital of Hamburg. The external surface of the left metacarpus was covered at this time with small blisters. On the middle finger was a large bleb, and next day, at the end of the ring finger, appeared another, the formation of which had been preceded by violent burning of the part. These blebs or blisters proceeded exactly as those of ordinary burns, with this exception, that their base presented a deeper red colour and their progress was less rapid, in so far as their complete development lasted usually twenty-four hours. On rubbing the diseased index finger with flannel, an acute sensation of burning was produced in the part.

Appetite was moderate, thirst intense, pulse regular, and the patient had no other morbid symptom except slight frontal headach.

The night between the 26th and 27th February was good, excepting that sleep was interrupted by some startings towards morning. No new blebs, but the left hand, the dorsal surface and fingers of which were painful to touch, retained a peculiar heat. The thermometer in this hand indicated 25° R. = 89° Fahr. and only 17° R. = 63° Fahr. in the right hand.

Several experiments were made on the patient with combustible materials, but no remarkable results were obtained; and the best electrometers brought close to the affected hand while the patient was placed on the isolator, remained quite insensible.

Loss of appetite and a bitter taste in the mouth were the only general symptoms.

This patient continued in the hospital till the 5th of May 1825, with very few symptoms deserving notice, except that on the 3d of March a new vesication appeared on the first phalanx of the index finger; one on the little finger on the 8th of March, *catameniis præsentibus;* one on the index finger on the 19th of March; and a little rheumatism in the left arm on the 1st of April.

In this case the flame and burning were seen by no competent witness. The girl told her story, and was believed. The blebs or vesications were seen no doubt by the physisician who treated her; and others. But that they were the effects of what is called spontaneous combustion, was an inference altogether gratuitous, and an inference resting on the statements of a hysterical young woman, whose imagination could invent things much more extraordinary than the existence of a blue flame on her arm, visible only in the dark.

That Margaret Frederika Heins felt in her arm and hand a sensation of heat and scorching may be admitted. That this heat was rather aggravated than abated by the use of cold water, is also very likely. But that it was accompanied with a blue flame, and a sulphureous odour, are altogether pieces of fancy and invention. Why should the smell have been that of sulphur? More likely, had any smell been emitted, it would have been that of phosphorus. But it is idle to attempt to explain what had no existence. The case was one of erysipelas or herpes, or pemphigus, in a young female, with disorder of the stomach.*

The fourth example of alleged spontaneous combustion to which Professor Bischoff refers, is one which is recorded by M. Richond de Brus, as having taken place on the evening of 19th April 1827, at Puy. The leading circumstances are the following.

M. D., aged 24 years, of middle stature, sanguine temperament, with black hair, rather lean than corpulent, and naturally very sober, went to the cathedral church of Puy, on the evening of the 19th of April 1827. There he remained but a short time; the intolerable heat which he felt there, compelled him to quit the church, and he went to the house of his brother. About half-past nine in the evening, this latter person was amusing himself by causing a small bit of sulphur to burn at the candle. This substance having melted and become inflamed, ran in that state upon his fingers, and caused rather acute pain. Some drops of the burning liquid adhered to his dress, and, it is added, inflamed it. The burning made rapid progress. His brother ran to him speedily, and laboured to stifle the burning, by enclosing the clothes in his hands. In this he succeeded, at the cost of a slight burning in two fingers, and a burned hole in his dress. But M. D., that is the same person who had extinguished the fire, felt in his hands acute pains, which made him utter loud shrieks and call for assistance.

A woman who ran up to him, perceived immediately that the hands of M. D. were covered with flames; they burned like candles, and the flames were bluish. At first it was thought that the flame was produced by the sulphur, and active measures were taken to extinguish them by pouring upon them cold water. This, however, was ineffectual. A poultice consisting of oil and flour only caused the burning to increase. At length they threw upon the burning parts cutler's clay.

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* Hecker's Annalen, 1825, band ii. p. 495; Nouvelle Bibliotheque Medicale, t. ix. 1845; Annales d'Hygiène, t. 40, 1851, p. 402.

M. D. ran to M. Richond, with a wild eye, red countenance, and the expression of despair in all his features, and, exclaiming that he was burning, demanded assistance. His hands were red and swelled, and a sort of vapour or smoke issued from them. M. Richond caused him to place his hand in a well or fountain, and then he experienced relief; the flames were extinguished, but very soon, at the distance of one hundred and fifty paces, he saw them return. When he reached home, he placed his hands in cold water, which was quickly rendered warm. Every time that he took his hands out of the water, he saw, he stated, a sort of fat flowing over his fingers, and the bluish flames immediately reappeared, especially if he placed his hands in a dark place.

The pains continued acute during part of the day; but they became less severe and pungent than they were at first. Upon the fingers large blisters or *ampullæ*, filled with reddish serous fluid, had risen. In several points the epidermis was entirely removed, and the true skin, denuded and greyish, appeared to be corroded. These injuries were dressed like a simple burn, and the patient returned to his home, five leagues from Puy.

Twenty-two days after the accident, M. Richond saw M. D., and found him then in a satisfactory state. Healthy suppuration of the parts had been established, and already several of the sores were healed. Subsequently all the injured parts were healed, but the nails had dropped off from several of the fingers.

M. Richond makes the observation, that if the flames had been perceived only immediately after the burning of the dress of M. D.'s brother, it might have been justly thought that they had been produced by some particles of inflamed sulphur adhering to the skin of the hands. But these flames resisted the affusion of cold water and prolonged immersion of the hands. They continued during the night and they were reproduced spontaneously a short time after the hands had been withdrawn from the water of the fountain.*

Upon this case Professor Bischoff remarks, that it is manifest the burning proceeded from the burning sulphur which the person burned had spilled upon his clothes, in attempting to extinguish by his hands the flames of the sulphur upon the dress of his brother.

Whether this be the true explanation we leave to intelligent readers to determine. The case, which was originally published in the *Archives Generales de Médecine*, has been admitted by M. Breschet, in his article on *Human* Combustion, in the *Dictionnaire de Médecine;* and so far as it is possible to judge without marks of doubt. The story, marvellous as it appears to be, viewed, at least, as an instance of spontaneous combustion, seems to be wonderfully silly. Throwing out of view the childish folly of a person between twenty-five and thirty amusing himself by burning sulphur at a candle, without any useful purpose, it seems either a piece of contradiction to say that the flames at first were not extinguished by cold water, and afterwards that they were; or it must have been that enough of cold water had never been employed for the purpose. It seems more probable that they had been extinguished, but that, as the sense of heat and burning which was left after an actual burn, was still tormenting the patient, he imagined that his hands were burning, when they were only the seat of the first sensations of scorching after being burned.

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* Archives Generales de Médecine, tome xix., p. 430.

But whatever inference he formed on this point, the case cannot with justice be quoted as an instance of spontaneous combustion. It is merely a severe burn, from the hands having been thoroughly exposed to the action of fire, and the clothes a little burned.

Bischoff remarks that not one of these alleged and represented facts has been reported by any scientific person, or any competent observer. Physicians have seen several of the subjects, as has been shewn, after the alleged catastrophe. Other cases have been the object of judicial inquiries, and have merely an appearance of certainty. But when the great difficulty of arriving at truth among the statements of ordinary witnesses, whose observations is biased by alarm, credulity, stupidity, and a love of the marvellous, is remembered; when it is considered how difficult in every inquiry it is to obtain clear and indisputable evidence as to the most common matters of fact; it seems perfectly reasonable to hesitate admitting inferences which are known to be at variance with the known laws of the physical world and chemical action.

It appears further, that in reference to the case which formed the subject of inquiry, singular mistakes and contradictions were emitted by different witnesses, which shewed how little their observation was to be trusted. The watch of the Countess, it appears, was burned, and did not go. All the commissioners and several others had this watch in their hands. They were compelled upon oath to say whether it had stopped at a certain hour. None of the witnesses could pronounce on this point with certainty; yet it was a point very easily determined. One person alone declared, without hesitation, that the watch had gone to the end of the chain, when it ceased to beat; this was the watchmaker. He was the only person of all who possessed this simple degree of knowledge, or thought of applying it.

Numerous contradictory statements were emitted by different witnesses upon the burning of the library-table, of the flooring, and of the dead body itself. Two females who had cleaned and dressed the corpse, asserted that one of the arms was wanting; and this assertion they supported by another statement, that they had not been able to put on the dead body one of the sleeves of the gown. All the medical witnesses knew most certainly that both arms were present.

It is consequently, not wonderful, argues Professor Bischoff, that upon a subject so obscure as that of spontaneous combustion, and as it is represented to take place, witnesses should substitute supposition and fancy for observation. At the time when most of these cases occurred, observers were incompetent in various ways; from ignorance, prejudice, credulity, and sometimes causes less venial. The appetite for news and for marvellous relations probably operates not with small effect; and newspaper writers, from carelessness, similarity of taste, from credulousness, or a desire to gratify this well-known appetite in the public mind, not unfrequently contribute to the same result. An example of this occurred so recently as the month of February 1850, in relation to a supposed case of spontaneous combustion, which was represented about that time to have taken place in a public house in one of the suburbs of Paris. The relation, published by the *Gazette des Tribunaux*, was reproduced on the 25th February 1850 by the *Journal des Debats*.

Xavier G. a house-painter of very intemperate habits, being drinking at a public-house at the *Barriere de l'Etoile*, with several associates, undertook for a wager to eat a lighted candle. Scarcely had he introduced the burning candle into his mouth, when he uttered a slight scream and fell down amidst the general stupefaction of the party. A blue flame played upon his lips. Attempts were made to aid him; but the assistants when they wished to raise the man, were seized with terror when they saw him burning internally. In half an hour the head and the upper part of the chest were charred. Two physicians summoned, declared that the man was the victim of spontaneous combustion. The bones, skin, muscles, all were consumed and reduced to ashes. Some parcels of dust in the place where the man fell down, were all that remained of the dead body.

This is followed by some general remarks, the tendency of which is to create wonder and terror at an unknown event of doubtful occurrence, and at variance with known physical laws.

Professor Liebig wrote to several of his scientific friends at Paris, making inquiries to ascertain the facts of this alleged case. Regnault first answered, stating that he doubted the whole facts and doctrine of spontaneous combustion, and expressing his belief that the story of the drunken painter in the Cabaret at the Barriere de l'Etoile was merely a piece of invention. M. Pelouze soon after added his belief that the story in the *Journal des Debats* was pure invention ; and finally a note from the Prefect of Police announced that the statement was completely imaginary : that, had it really occurred, the death of the painter must have come under his cognisance and inquiry ; that after minute researches which he had directed to be taken, all had proved ineffectual ; that he found the story had been copied from the *Gazette des Tribunaux* ; and that he had made inquiry at the office of this journal, and from the mode in which the question had been received, it was not difficult to see, that the narrative was a fabulous story, invented to please the public taste.

When we come to consider the actual possibility, and the degree of facility of burning a dead body, still greater difficulties to this hypothesis appear. A dead body in which there is 25 per cent. of water does not inflame, and when inflamed, it continues not to burn. Professor Bischoff maintains, that if all the solid parts of the body, namely the bones, the skin, the tendons, and the muscles, were placed in a pile to be burned, while the water contained in the body was in a vessel, if these solid parts were made to burn, the consumption of all would not produce heat sufficient to convert the water into vapour.

One argument which has been applied with considerable force, to prove the evidence of spontaneous combustion, has been derived from the circumstance, that many, if not all, the victims of this alleged mode of dissolution are addicted to the constant use of spiritous liquors. It has been argued and stated, both by philosophers, and above all, by popular writers, that the system of the drunkard becomes so impregnated with spirits, that it acquires the property of burning spontaneously. The argument, and the application have been further employed by the advocates of temperance to warn drunkards against indulging in the habit, in order that they may avoid so dreadful a death. However excellent the practical object may be, it is founded on a number of fallacies and mere creations of fancy.

Alcohol is doubtless of itself inflammable ; but it has never been proved that alcohol taken into the human body is capable of this species of inflammation. It is in no instance drunk in such purity and strength as to render this possible ; and if it were drunk in such purity and strength, and in sufficient amount, it would either produce speedy death in a way totally different, or symptoms of narcotic poisoning. This has been proved and known in many instances. When taken in dilution it is quickly absorbed by the vessels of the stomach and duodenum, is circulated through the liver and lungs, and being mixed with the blood in this diluted state, its elements are expired with the air. When taken in purity, the quantity capable of being taken without producing fatal effects is too small to be capable of rendering either the breath or the body inflammable.

The subject, however, of the introduction of alcohol, pure or diluted, into the human body, gives rise to various questions not so easily answered as is usually supposed. Of the two conditions which have been already stated of its admission in the pure and its admission in the diluted form, the latter alone has given rise to much discussion. One point which has been the subject of much inquiry is, whether the presence of alcohol can be detected in the blood and in the tissues. Dr Percy, it is well known, obtained alcohol, or at least a fluid with alcoholic qualities, from the brains of dogs, into the vessels of which spirit of wine had been injected. But this result is at variance with the results obtained by others. Thus Professor Pommer of Zurich performed similar experiments, yet did not find alcohol in any of the tissues or fluids. M. Devergie quotes the instance of a child of six years, who died in the course of twenty-two hours after having swallowed a quantity of strong spirit, and in the organs of which, distillation did not prove any traces of alcohol. MM. Bouchardat and Sandras have not been able to discover any traces of alcohol in any secretion except in the pulmonary exhalation. This circumstance is in accordance with the experiments of Chossat, who ascertained with as great certainty as the subject admits, that alcohol is eliminated by the lungs. It is a considerable time since Wöhler proved the absence of alcohol in the urinary secretion in circumstances where the spirit is taken in quantity.

On the other hand, the experiments of Magendie, Orfila, and Rayer, shew that alcohol is rapidly absorbed by the veins of the alimentary canal. It is scarcely possible to doubt that in the case of habitual drunkards, the liquor is taken up directly by the bloodvessels, and follows the cours of the circulation through the different organs. But it is to be remembered that the spirit drunk in this manner is always more or less diluted, and is never of that purity and strength which spirit of wine is. Were it of this purity and strength it would speedily cause death, partly by its power of causing coagulation of the blood, partly by its violent constriction of the tissues, and partly by its speedy effect on the nervous system, the actions, powers, and influence of which it suddenly suspends and annihilates.

To suppose, therefore, that any amount of alcohol or alcoholic liquor could cause either the breath exhaled to take fire on the animal tissues being impregnated with it, is to admit an amount of accumulation of this poison in the system totally incompatible with the continuance of life. M. Bischoff states that he is convinced that though a dead body were soaked in spirit, it would not in consequence of that become combustible. He took parts of a dog, into the arteries of which alcohol had been injected at 92°. These parts did not burn either at the flame or exposed upon coals. They became roasted and charred, but ceased to burn so soon as they were withdrawn from the fire.

It must not be denied, nevertheless, that this experiment furnishes no satisfactory conclusion, against the hypothesis of spontaneous combustion, such as is supposed to be induced by habits of incessant drinking. It is possible that under the long continuance of these habits the whole organism may be modified in such a manner as to become, upon the application of certain causes, combustible. But in this case it is at variance with everything known to imagine that the alcohol in the body is the agent or means of the combustion. It cannot be the alcohol that burns. It must be an animal body converted into a combustible body.

As to the story so often told of persons represented to emit flames by the mouth, either in a state of intoxication or in consequence of the long-continued use of spiritous liquors, it is a mere fable for children. It is doubtless possible, that spirits might in the act of swallowing be set on fire by a light or match. But this is not what is represented to take place, neither does it touch the question at issue, which is that of the breath becoming inflamed spontaneously,or when it reaches the atmosphere. It is still less possible to conceive the breath to take fire, whatever may be the habits and the alcoholic impregnation of the individual. Long before this could take place, life must have been extinguished by the poisonous action of the accumulated alcohol.

Professor Bischoff relates the following experiment. He injected into the crural veins of a dog a small quantity of ether. His colleague, M. Liebig, brought a match near the nose of the animal. Immediately during expiration they saw a flame, which was extinguished during inspiration, and was again kindled by the match on the following expiration. It went out once more, and was rekindled as before. In a few minutes the animal expired, though the ether injected was in small quantity. From these phenomena Professor Bischoff infers that, when volatile substances reach the lung, with the current of blood, the lung isolates them and expels them with great rapidity. They do not appear to be accumulated.

Alcohol, in like manner, when it is introduced in large quantity into the circulation, is eliminated by the lung. But even when the air is saturated with it, it is no longer inflammable. M. Bischoff injected alcohol at 92° into a dog; yet he could not cause the expired air to take fire. If the movement of expiration be made through a sponge soaked in alcohol, it is impossible to inflame the air which escapes. The same experiment performed with ether produces flames.

From these facts he concludes that if any person were drinking a large quantity of spirit, and if he discharged it by respiration, his breath could not catch fire, as is vulgarly supposed.

Lastly, in order to shew the possibility of the body of the deceased Countess having been burned by ordinary means, and common combustible materials, and communicate some idea of the effects of such burning upon a dead body, Professor Bischoff gives the report of an experiment performed at Giessen, with the view of obtaining correct information on various points which had been called in question.

In an apartment attached to the Anatomical Theatre at Giessen, he arranged, on the 28th February 1850, a scaffolding or pile of bricks and wood, imitating the secretary which had been burned in the apartment of the Countess of Goerlitz. At the distance of from one foot and a half to two feet from the pile, he placed, upon four boards, the dead body of a man, laid on the back, with the head turned to the right, so as to have the left side and the skull particularly exposed to the fire. The lower part of the body was out of the pile. Athwart this body he placed the body of a dog, which was killed by injecting ether into the veins, and the arteries of which were then filled with alcohol at 92 degrees. Having set fire to the wood by means of a few burning coals, at ten minutes past eleven, he shut all the doors and outlets. Very soon much smoke was disengaged, and, getting over all barriers, diffused itself through the whole house. Speedily through the doors were heard, not only the cracking of the burning wood, but the hissing produced by the roasting of the dead bodies. The heat was so intense that one door nearest to the fire was burning externally, though oak planks had been placed over its inside, in order to preserve it.

At forty minutes past eleven, that is after half an hour, Professor Bischoff opened one door, and when part of the smoke cleared away, they saw the heads of the two bodies enveloped in a blue flame. But so soon as a second door was opened, and a current of air was established, the pile and the heads threw out flames; and the latter continued to burn, though they were removed from the pile; and it was requisite to apply wet cloths to extinguish them.

The state of the bodies was the following.

All the left side of the head was black, and covered with very porous charcoal. The head, which had been inclined to the right, was, by the retraction of the skin and the muscles, brought to median line, and inclined a little to the left. The lips were separated by reason of the desiccation, the teeth firmly implanted, and the jaws strongly contracted. The left half of the soft parts of the nose, and the eyelids of the left eye, were charred. The orbit was dried, but its shape was preserved. The left temporal muscle charred was detached from its superior insertions; and the lower portion, protected by the bones, retained still some fragments of flesh. The scalp, on the left side, was charred. The skull was denuded on the right side, because the integuments had all retracted to one inch of the ear. Towards the occiput the skin was charred at the upper part, destroyed, and embrowned at the base. The right half of the face was blackened, greasy to touch, and burned to a great depth in the orbit. On the *ala nasi* of this side were two brown dry phlyctenæ.

The bones of the skull had retained their shape. But the inner tables of the left parietal bone, the left half and part of the right of the frontal bone, the upper margin of the squamous portion of the temporal bone, and the external surface of the large wing of the sphenoid bone, were converted into friable brittle charcoal, so that almost everywhere appeared the diploe burned. The zygomatic extremity of the frontal bone, and the frontal extremity of the zygoma, were in great part charred, though retaining their shape. No suture was separated. Though the external table of the bones on the right side injured by the fire presented everywhere fissures and rents, the diploe and inner table presented none. No fissures were observed in any other part of the skull, even where the soft parts were most easily detached.

As the head of this body was burned in a less degree than that of the Countess of Goerlitz, the experiment was continued on the following day, Friday, the 1st of March 1850.

After arranging the combustible materials nearly as on the previous day, and placing the body in the same position, but three feet farther off than on the previous day, all the doors were shut, and the fire was kindled. The doors were opened at the end of one hour and a half, when sounds indicating the action of the fire had ceased to be heard. The combustible articles were consumed; but the head had been placed too far from the fire, so that the burning had made no progress, and it was requisite once more to begin the experiment. The same arrangements were made; but the body was placed two feet from the pile. The experiment was commenced at half-past one, and concluded at half-past three. The combustibles were at this time destroyed. Combustion of the body had made remarkable progress. The head was in the following state.

The skull retained its shape; but the soft parts of the left side of the head had completely disappeared. The skin, the ears, the nose, eyes, lips, cheeks, the muscles, especially the masseter, the temporal, and the buccinator, had disappeared, and in their place was left a little charcoal.

On the right side, the bones were still covered by soft parts, and even by skin, with the exception of the parietal bone, the temporal bone, the greater part of the frontal bone on the right side, and the large wing of the sphenoid bone. The skin, though retracted and burned, covered the right masseter and buccinator, as far as the angle of the mouth, and extended towards the zygomatic arch, and the angle of the eye. The right ear was in the same state. The right temporal muscle was detached from its insertions, and had, with the skin, been retracted as far as the superior margin of the zygomatic arch. The muscle was not charred but roasted. Of the right eye, the fissure of the eyelids was still distinguishable. On the left side, eyelids and eye were removed from their connections, drawn out, and burned.

The nose and the nasal cartilage on the right were equally destroyed. At the posterior part of the skull, the soft parts were charred and destroyed on the left side, as far as the nape of the neck, beyond the median line. On the right side, they were detached from the bones and retracted, so that the right side of the occipital bone, the temporal bone, and the posterior angle of the temporal bone, were denuded.

The soft parts retracted formed upon the right mastoid process a shapeless mass, half charred, on which, however, remains of hair were distinguished. At this part the burning of the bones of the skull was considerable. It extended from the semicircular line to the external occipital tuberosity. The following bones retained their white colour and their structure:—the right half and the base of the occipital bone, the right temporal bone, the part of the parietal bone included between the semicircular line and the squamous suture, the surface of the frontal bone directed to the temporal fossa, the whole malar bone, and probably a great part of the right half of the upper and lower jaw-bones, the *os unguis*, and the cribriform plate of the ethmoid bone.

Among the parts injured by the fire, the external table of the left frontal bone was in great part wanting, and the charred fragments were previously detached from it, so as to lay open the frontal sinuses. The diploe and the external table were charred, and traversed by two yawning fissures, one of which was directed towards the frontal suture, the other towards the supra-orbital region. The coronal suture was uninjured. The external tables of the left parietal bone were preserved in several spots; but they were whitened by the fire, as were also the parts of the diploe which were exposed. Towards the parietal tuberosity, the bone was traversed in three spots by fissures, from which had escaped fluids which had been charred externally.

The cheek bone of the left side had preserved its shape; but it was burned white, and rent, so as to fall in pieces on the slightest touch. The zygomatic process of the frontal bone, the superior maxillary, and the temporal were in the same state.

The squamous portion of the temporal bone, the mastoid process, and in general all the visible parts of this bone, were whitened, rent, and friable.

The left half of the occipital bone, mostly burned to whiteness, was traversed by fissures. The superior maxillary of the left side was charred; the zygomatic process detached. The alveolar processes were blackened, as were also the teeth, which were split and loosened. The anterior surface of the superior maxillary bone was rent, and the *antrum* was exposed. The nasal bones, the lachrymal bone of the left side, and the plates of the ethmoid bone were charred. The inferior maxillary bone, from its base to the angle, and the coronoid process was burned to whiteness, and furrowed by fissures; the *alveoli* were charred; the teeth split and loosened. None of the bones burned to whiteness were without marks.

The head was completely inclined to the left, in consequence of the retraction of the soft parts, which were partly charred, and partly greasy to touch.

In the chest, the fire had destroyed on the left side, the remains of the muscles of the chest, the ribs, and the collar bone. It is proper here to observe, that the muscles of the chest had served for an anatomical dissection, and that the arms and scapulæ had been severed from the trunk; but the muscles had been preserved and covered by the skin and a shift. The breast bone was charred on the left side.

The quantity of wood which had been burned in the secretary of the Countess of Goerlitz was computed at 67 cubic inches at least of fir, not including the flooring and the sides of the secretary. The knocker of the secretary was counted as being of fir. The quantity of wood employed in the first experiment was greatly less than this; and it is only by adding the combustible materials employed in the second experiment, that the amount of 67 inches is attained. But the fuel which served for the third experiment, must be considered as excess. We must take into consideration, the fact that, from the fuel of the first experiment should be deducted a quantity or quantities of charred wood which was extinguished by fire.

The skull thus treated was whitish-gray in general; the other parts of the head were of a deep black, like charcoal. The skull was so friable and fragile, that when slightly struck with a chisel, the cranial vault came away in fragments and shewed the brain, which was a glistening brown colour. In this state the head was greatly diminished in volume, and presented the aspect of a blackish mass, in all respects like the head of the Countess. It was impossible for any unpractised eye to recognise the different parts of the head, or the mouth, the teeth, or the tongue.

A pane of one of the windows was also exhibited. The glass was covered with the deposit similar to that which covered the glass of the mirror in the apartment of the Countess; and it exhaled a strong and offensive odour.

Another experiment was performed on the 23d February 1850, by burning by means of alcohol the head of the dead body of a female, aged upwards of 50 years, who had been dead two days.

The operation of setting fire to the alcohol was commenced at 35 minutes past two P.M., and was continued till 51 minutes past three, one hour and fifteen minutes. At first there arose a little smoke which was disagreeable to those not accustomed to medical inquiries. Afterwards, there was felt in the chamber only the smell of the alcohol in combustion. This was produced by a thin vapour.

During the past half hour, the temperature rose to 22° R. =82° Fahrenheit, and continued to the end of the operation.

The soft parts were pretty well burned, and also the bones. And both soft parts and bones were charred very much in the same way as in the previous experiment.

From these facts Professor Bischoff and his associates deduced the following conclusions:—

1. Phlytenæ, even though they present a reddish base, do not prove that the burnings have been made on the living body.

2. The burning of the head of the subject of the experiment differs in no essential circumstance from what was observed on the body of the Countess of Goerlitz.

3. From the physical state of the parts consumed and burned, there is no reason to ascribe the burning of the Countess of Goerlitz to any other cause than the action of ordinary fire externally applied.

4. The experiments prove that in three quarters of an hour the head of the Countess might have been reduced to the state in which it was found at the time of examination on the 14th of June 1847.

5. It is further proved, that in a chamber very low, and in the circumstances above specified, men might, without inconvenience be present at the burning, which, *a fortiori*, would be still more easily done in a large chamber.

The person who burned the body of the Countess, had it in his power to open not only one, but three doors, and if necessary, even a window situate over the court, since no person could have perceived a very thin vapour. The guilty person had also every facility for getting in and coming out. The proof, therefore, which it has been wished to derive against the perpetration of a crime, from the impossibility of breathing in consequence of the smoke produced by the combustion, falls to the ground completely refuted.

Besides the experimental trials now mentioned, Drs Graff, Buchner, Joacheim, and the surgeon Freniard, on the 28th of February 1850, burned the head of a sheep by means of oil, in an apartment seven feet high, twenty feet broad, and twenty-two feet long.

In a vessel seven inches wide, and three inches and a half deep, they poured what they call half a measure of oil. Round this vessel they arranged chips of fire-wood in such a manner as to rise above the surface of the oil between half an inch and one inch, and to serve as a sort of wick or conductor.

At sixteen minutes past two, they placed above the oil-vessel a tripod, and set on this the head of a sheep deprived of the fat, about five or six inches above the surface of the oil.

At twenty minutes past two, four minutes after the commencement of the operation, drops of fat oozed from the head and blackened the parts turned to the fire.

In fifteen minutes more, the wood, not producing a sufficient flame, was removed, and into the oil was thrown a handful of cotton, and half a handful of linen thread. After this the flame became a little more intense. More smoke arose from this species of fire than from the alcohol; but it was quite easily borne.

All the experimenters continued around the fire, attended by a servant.

At fifty minutes past two, that is after thirty-four minutes, the oil was boiling.

At twenty minutes past three, that is sixty-four minutes from the beginning of the experiment, the experimenters added to the oil two spoonfuls of alcohol. This caused an explosion similar to that of a pistol-shot, and the flame blazed up above the head; but it quickly returned to its previous condition.

At twenty-one minutes past three the experiment was finished, after lasting one hour and five minutes.

Three ounces of the half measure of oil were left unused. The skull was not opened nor cracked, but it was completely burned, and partly calcined. The following conclusions are given.

1. The head of a sheep may be burned, with half a measure of oil, a little cotton and tow, in the course of one hour, so as to be reduced to one-third of its original volume. This result would have been obtained more speedily, if, from the beginning of the experiment, tow had been employed instead of wood.
2. The addition of water or alcohol increases the flame for the moment twofold or threefold.
3. With double the quantity of oil, a human head, with the neck and chest, may be burned in the same manner, and in the same time.
4. Oil produces more smoke than spirit; but this smoke does not embarrass respiration, or cause coughing.
5. Upon all the surrounding objects much soot is deposited.

These experiments may be allowed to prove, with little room for doubt, the fact, that a human body, or an animal body, does not burn of itself, without the aid of combustible materials applied externally; and that, with the aid of combustible articles, it is possible to produce, in every form and degree, the full effects of the action of fire.

Much importance has been attached to the peculiar qualities of the smoke and soot produced after alleged instances of spontaneous combustion. This smoke and soot, however, possesses no peculiarities, except in so far as it is produced by the burning of animal matter, and is therefore mechanically mixed with animal fat, and exhales an animal odour. It most probably contains much more sulphate of ammonia than does vegetable soot, and probably also muriate of ammonia, from the decomposition of the chlorides. The presence of this peculiar soot, therefore, however offensively it may smell, should never again be adduced as a proof of spontaneous combustion.

The facts and arguments now mentioned ought to have been quite sufficient to prove, that the burning of the body of this unfortunate lady was in no way connected with what has been called spontaneous combustion, and that it was merely an ordinary case of the destruction of a human body by fire, from ordinary burning materials externally applied. Professor Liebig gave, however, another well-reasoned report, chiefly against the credibility of the general doctrine of spontaneous combustion, and shewing the real foundations on which that doctrine rests. This report our limits permit us not at present to reproduce; but at some more convenient season we may recur to it.

Meanwhile it may be proper to mention, that though at first this affair appeared to be involved in impenetrable mystery, and gave rise to great contradiction in the medical evidence, yet the cause of truth and justice, after the lapse of three years, prevailed. John Stauff, of Oberohmen, was found guilty of the assassination of the Countess of Goerlitz, and was condemned to perpetual imprisonment during his natural life. In the prison of Marienschlop this man made a full confession of his crime. He declared, that having entered the chamber of the Countess, in order to inform her that he was going out, and not finding there any person present, while the secretary of the Countess, containing money and valuable articles, was open, he could not resist the temptation to thieving. The Countess meanwhile entered, and beheld him in the act. He immediately engaged in a struggle with her, and strangled her. He then placed the dead body on a couch or sofa, near the secretary, and having surrounded it with combustible materials, he set them on fire, in order to conceal all traces of the murder which he had committed.

The temporary success which attended the endeavours of John Stauff, is shewn by the obscurity and mystery which so long hung over this transaction, by the discordant evidence at first given, and by the elaborate inquiries made to discover the truth. The ingenious pleadings of Dr Siebold, founded, however, upon many assumptions and suppositions, contrasted with the reports of Professors Bischoff and Liebig, and finally corroborated by the confession of the criminal himself, form as remarkable a history as the records of crime contain, and furnish as useful a commentary upon the service of medical evidence as can well be conceived. The murder of the Countess appears not to have been exactly what is called premeditated, unless in so far as the man who is prepared to take the property of his fellow-creature, is often tempted violently to destroy life at the same time. But, in strangling the Countess, in order to conceal the act of theft, the murderer thought it necessary to burn those parts of the body which must have borne marks of his violence. Had it not been for the sound judgment and patient inquiries of the Medical Commission, aided by Professors Bischoff and Liebig, John Stauff might have escaped and been left at large, with nothing but suspicion on his character, and would, no doubt, have been encouraged, by the impunity of the first murder, to commit a second. His conviction and life-long imprisonment have not only prevented this result, but they must be serviceable in shewing to others, who desire to plan, or may be led to commit, crimes of this kind, that they have no chance of finally escaping the hands of justice.

In giving the foregoing abstract of the arguments and reasonings of Professor Bischoff, it has been our endeavour to represent them substantially as they are given by the author; and we have at the same time studied to avoid taking a side, unless so far as the facts upon which the reasonings are founded may justify such a course. The report of the Professor, which is stated to be oral, is not so methodical as could be desired, and the facts founded upon are given without references, except of the most general character. This renders the report, as given in the *Annales d'Hygiène*, of little service as a scientific document, and almost useless to those who wish to examine the evidence.

For the reasons now assigned, it has been felt requisite to give short, but correct statements of the facts, with accurate references to the original authorities. In so doing, the order of the statements of facts has also been changed; and we have observed, as nearly as possible, the chronological succession, as the one most natural, and most likely to be useful to those who wish to study the subject with any attention.

ART. VI.—*Instance of Reported Spontaneous Combustion in Morayshire.*

In the *Elgin Courant* for the week ending Saturday, 5th June 1852, is recorded an instance of what is called *Awful Death*, as having taken place on the previous Saturday, that is, 29th May 1852. The body of a man named John Anderson, by occupation a carrier, Whytemyre, was found lying in a field near the Hardmuir Toll Gate, fearfully burned, it is stated. The man was seen in a state of intoxication, and smoke arising from his clothing; but before assistance could be rendered, the man was in the agonies of death.

It is supposed that death was the effect of spontaneous combustion, the alcoholic spirit, it is added, with which the man's body was charged, having taken fire; or by its coming in contact with a spark from the pipe with which he had been smoking.

This is the manner in which cases of spontaneous combustion are recorded. A man or woman drinks until stupefaction begins to overtake him or her; then the party takes a smoke; some of the embers fall upon the clothing, which, if of linen or cotton is sure to take fire; the individual, insensible and helpless, is unable to make efforts to extinguish the flames; the shirt or clothing is burned, and with it the body. Then when the next spectator comes up, the body is found burned and lifeless; and this is spontaneous combustion.

If the fact of Spontaneous Combustion is to be established in any given case, it must be on better evidence and different grounds than those now enumerated. The individual should be neither stupified with drink, nor have been smoking a pipe. Every one knows how easily and how often masses of dry peat and hay, and similar articles, have been set on fire and reduced to ashes by careless persons smoking a pipe. All smokers are careless, stupid, and reckless; and hence they and drunkards are more frequently burned, and cause and suffer injuries by fire more than any other members of the community.

It has been uniformly said that females are more frequently the subjects of Spontaneous Combustion than men. Are they not more frequently the subjects of common burns than men? This is scarcely a matter of doubt. The reason is, that the clothing of females more readily catches fire and burns more rapidly than that of men. The argument, therefore, goes for nothing.

PART II.

CRITICAL ANALYSES.

ART. I.—*The Bath Waters: Their Uses and Effects in the Cure and Relief of various Chronic Diseases.* By JAMES TUNSTALL, M.D., Graduate of the University of Edinburgh, Physician to the Eastern Dispensary of Bath, lately seven years Resident Medical Officer of the Bath Hospital. London, 1850. Post 8vo, pp. 144.

"When I entered the Bath Hospital," says Dr Tunstall, "as its Resident Medical Officer in April 1843, I saw before me a number of patients labouring under a few diseases of rare occurrence in the practice of individual medical practitioners, which were there collected from various parts of the kingdom. The sick of the palsy, the leper, and those with the withered hand, were there together; and I observed immense benefits produced by the Bath thermal treatment alone, in diseases which ordinarily baffle the medical skill even of the most profound.

"I then sought the baths and pump-room, and found them comparatively deserted, and naturally asked why the poor should derive so great benefit, while, by the middle and upper classes of society, the springs were so lightly valued? The truth was soon apparent. With the exception of the short notices by Dr Granville and Dr Edwin Lee, in their works on Spas and Mineral Waters, no treatise upon the Bath waters had issued from the press for a period of nearly thirty years."—(Preface, p. i.)

In this mode Dr Tunstall explains the fact of the comparative desertion of Bath and its waters by the middle and upper classes during the time to which he refers. We acknowledge that we cannot agree with the author, either as to the cause which he assigns, or the adequacy of its influence in the production of the effect. Of the correctness of the fact he has the best means of judging. We were aware that Bath, and, to a certain extent, almost all the English watering places had been of late years much less frequented than upon former occasions. The explanation, which to us appears to be the correct one, may be conjectured from the statements which we have made in the preceding volume, in the article on the work of Dr Sutro. All those persons who had at command time and sufficient pecuniary resources, were desirous to visit the Continental baths, whether they were actual invalids or only fancied themselves ill; and, as to visit any one of these baths, especially those in the neighbourhood of the Rhine, was quite as easy almost as to go to Bath; as the places and scenery were new; as the Rhine was a magnificent river to sail upon; and as living was in point of fact rather less expensive in the continental watering places, whether in lodgings or in hotels, and certainly, if properly managed, by no means more expensive;—when all these considerations in favour of Wiesbaden, Ems, Homburg, Carlsbad, Baden-Baden, or Kissingen, were thrown into the one scale, and the arguments for the quiet city of Bath were thrown into the other, the latter with the great majority of persons and families fairly kicked the beam. Visitors accordingly no longer resorted to Bath in the numbers in which they had previously flocked to that city.

A member of a family becomes unwell; and the ordinary medical attendant, with the physician, either see that they can do no more or become tired with the case; the patient meanwhile daily becoming worse and weaker; and to bodily suffering is added mental despondency. At length one or both parents propose to the physician or medical attendant the question, what is their opinion as to the effects of some watering place; and forthwith, as to some of the German baths. The answer is given rather in doubt, and unfavourably. But another physician gives an opinion either decidedly or in such a manner, that it is interpreted into a sanction. Within eight days the whole family are steaming up the Rhine. No wonder that Bath was deserted by those in easy and affluent circumstances.

Such is the way in which Bath has been deserted. The system now hastily described commenced fully twenty years ago, and it has been proceeding in full vigour ever since, receiving perhaps a slight check during the turbulent and portentous years of 1848, 1849, 1850, and 1851; but never wholly interrupted. It may be safely asserted that during the ten years between 1838 and 1848, almost all who could spare time and the other conditional article, visited for a shorter or longer time one or other of the German watering-places. It remains to be seen whether the governments of the German states, whose subjects derived such a steady, abundant, and easily collected revenue from the restless inhabitants of Great Britain, will be so blind to their own interest, and so indifferent to the interests of their subjects, as to adopt measures which may render it either unsafe or disagreeable for the English visitors to travel in these regions.

Another circumstance not unimportant in the decision of the inquiry, candour prevents us from omitting to state. Though England is, considering its size, not badly supplied with mineral springs of different kinds, the number of thermal or hot and warm springs is very small, compared with those in continental countries, and especially in Germany. Beyond Bath, the hot well at Bristol, or rather Clifton, the tepid springs of Buxton, and the rather cool waters of Matlock; there is no warm or hot spring in the whole island. In Germany, on the other hand, hot and even boiling springs are numerous and frequent. First come those of Aachen and Borcette; next the hot alkaline waters of Ems; then the boiling springs of Wiesbaden, so hot that fowls are scalded by the water of the Kochbrunnen; then the hot or warm waters of Baden, of Carlsbad, of Wildbad, of Ppeffers, of Gastein, of Toplitz, and others too numerous to recount. There is something which so strikes the senses and impresses the imagination in beholding large quantities of hot or boiling water gushing from the bosom of the earth in copious smoking streams; something so calculated to set the mind a working, as it were, that, combined with the scenery, and the geological aspects of the country, it is not wonderful that many persons should visit these places both from curiosity and from other causes connected with, or dependent on, those relating to health. Nor is it to scientific persons alone that this admiration is confined. It is impossible for any person with any degree of imagination and mind to look upon the Kochbrunnen of Wiesbaden, or the smoking springs of Aachen and Carlsbad, without being led to think on the cause of these boiling fountains, and to propose to himself the question why these fissures in the globe have continued to pour forth such quantities of hot water for centuries, apparently without diminution or interruption.

Dr Tunstall has, further, not stated his case in terms so strong and so clear, it appears to us, even in his own view, as he might have done. Two authorities in favour of the use of the English waters he has omitted. Dr Granville, whose volumes appeared in 1841,* and Mr Lee, who published first in the same year, and again in 1848,† are not the only writers who have studied to explain the merits of the Bath waters, subsequent to the appearance of the excellent treatise of Dr Barlow in 1822.‡ Sir Charles Scudamore, in a short Treatise on the Composition and Medical Properties of Mineral Waters, published for the second time in 1833, gave a correct and instructive account of the Bath waters, and the class of cases in which they were most likely to prove beneficial.§ And the indefatigable Dr James Johnson gave, in 1843, a short sketch of the Bath waters and their hygienic and therapeutic applications, in his Excursion to the Principal Mineral Waters of England.|| Here, therefore, are four treatises published since 1822, not exclusively indeed on the Bath waters, but on the use of the English mineral waters, for the information of patients, and probably the guidance of physicians. Yet, amidst the strenuous efforts of these four advocates, notwithstanding the sensible and practical manner in which the third author illustrated the therapeutic operation of the Bath waters, and the animated appeal of the fourth author, the English invalids, visitors, and travellers continued to flock to Aachen, Ems, Wiesbaden, Carlsbad, and Kissingen; a few perhaps to Vichy and Pau; and Bath became like a city comparatively abandoned.

These facts we recal, for the purpose of shewing, that nothing short of a miracle, we believe, could have kept up the

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* On the Spas of England. By A. B. Granville, M.D. 3 vols. Post 8vo. London, 1841.

† On the Mineral Springs of England. By Edwin Lee. London, 1841. 12mo. The Baths and Watering Places of England. 2d Edition. London, 1848. 12mo.

‡ An Essay on the Medicinal Efficacy and Employment of the Bath Waters, illustrated by Remarks on the Physiology and Pathology of the Animal Frame, with reference to the Treatment of Gout, Rheumatism, Palsy, and Eruptive Diseases. By Edward Barlow, M.D., &c. Bath, 1822. 8vo. See also Edinburgh Medical and Surgical Journal, vol. xix. Edinburgh, 1823, p. 305, *et seq.*

§ A Treatise on the Composition and Medical Properties of the Mineral Waters of Buxton, Matlock, Tunbridge Wells, Harrogate, Bath, Bristol, Cheltenham, Leamington, Malvern, Isle of Wight, Brighton, and the Beulah Spa. With Instructive Observations on the Drinking of the Waters and the Use of the several Baths. By Sir Charles Scudamore, M.D., F.R.S., &c. The second edition, corrected, and much enlarged. London, 1833. 8vo, pp. 215.

|| Excursion to the Principal Mineral Waters of England: a Journal of Health and Information. By James Johnson, M.D., &c. London, 1843. Post 8vo.

stream of visitors to Bath, while such a current was flowing strongly during summer and autumn in the direction of the different German watering-places. It was not the want of books upon the subject. Several books, by no means deficient in the requisite qualities, as we have shewn, were published; and even individual monographs on particular places were not wanting. Yet they were not read; or if read, they made no impression, amidst the stronger excitement of continental travelling. So completely had the English got into the habit of making this annual expedition into the German States, and with such regularity and in such numbers did they migrate for a long series of summers, that, while during the months of June and July, they were most numerous in the different towns on the route, the hotel-keepers of Wiesbaden, Baden-Baden, and Carlsbad, were wont to speak in confident terms of the great accession of visitors which they would certainly receive, as soon as Die Grosse Englische Parliament arose in August.

In the meantime, this habit of annual migration into the German States, will most probably receive a serious and considerable check this season, if it have not so done already. An Englishman, especially with ladies, likes to travel without dread of interruption, obstruction, harassment, and anxiety about personal liberty. This he can scarcely do this ensuing season; and, indeed, if we are not misinformed, there were unequivocal signs last year, that something was not right. Passports were not inspected and returned with the same promptitude and facility, with which they were wont to be in the years previous to 1848. Interruptions were more frequent and serious, delays were longer, and in short, everything shewed that the English traveller was something out of place. If this system be continued, and if the Austrian Government carry into effect their beneficent intentions towards English travellers, as they announced some time ago they were resolved to do, it requires very little foresight to predict, that our restless countrymen will direct their steps to some other quarter. They will visit Greece, as they did when the war closed the Continent against them, Turkey, or Egypt. What is more to the point, many will probably be induced to visit the English watering-places, and spend their time and money among their countrymen. Those particularly who would be deterred by a distant expedition up the Mediterranean and to the Levant, and the probable hazards of striking on the Skerki rocks or the steam-ship taking fire; those who are seriously invalids, will prefer the more accessible places at home. But, what will please Dr Tunstall and the physicians of Bath, the invalids will assuredly find their way back to that interesting city. As we think the work of Dr Tunstall may be not without effect in contributing to this desirable result, we propose to devote a few remarks to its claim on public attention.

We begin by remarking, that we have always thought, that amidst the passion for visiting foreign watering-places, Bath was unjustly neglected by the natives of this country. Not only was it at one time allowed to be a mineral water of undoubted efficacy; but even before the composition of its waters was perfectly understood, it was admitted that the internal use and the external employment of its waters produced in certain classes of cases of ill health, the same beneficial therapeutic effects, without the debility of ordinary hot baths, which have been since so confidently ascribed to several of the continental thermal waters.

On one point we have always entertained the same opinion. In estimating the physiological and therapeutic effects of the Bath waters, their true chemical composition has not been perfectly understood; and physicians have blindly, and rather thoughtlessly taken the results of analyses as indicating the true chemical constitution of these waters. Look, for example, at the analysis of the King's Bath water, as given in the book of Sir Charles Scudamore. In one pint of sixteen ounces, there are contained;

Muriate of lime, ...	1·2 grains.
Muriate of magnesia, . . .	1·6
Sulphate of lime, ...	9·5
Sulphate of soda, ...	0·9

Now it is here perfectly clear, that the sulphate of lime can do no good, and probably would do harm; and that the other ingredients as stated, could not produce any very remarkable beneficial effect. These, it must be allowed, are the result of analysis. The reasonings, however, and explanations of Dr Murray, which were known to and mentioned by Sir Charles Scudamore, would lead to the conclusion, that in the water as it existed in the spring, before analysis, the ingredients were sulphate of soda in considerable quantity, sulphate of magnesia probably also to some amount, and muriate of lime and magnesia perhaps in small amount. It is singular, that though Sir Charles Scudamore is, as we have stated, quite aware of the inferences to which the theoretical views of Dr Murray must lead, yet he does not attempt to present what we call a rectified analysis, when he afterwards gives the most recent chemical composition from the labours of the late

Mr Richard Phillipps. This chemist found, as most persons must have observed, that the water contains a good proportion of carbonic acid, 1·2 cubic inch in sixteen ounces; he confirmed the inference drawn by Sir George Smith Gibbs, that the water contains silica; and he shewed that iron was present, in the form of oxide, though in smaller proportion than at first represented. Now these are facts most important. We know now in what form the lime was contained in the water. It was held in the form of supercarbonate by the carbonic acid. The muriate of lime, therefore, may be discarded, notwithstanding Dr Murray's confidence in that article as a therapeutic agent. The composition of the Bath water, then, would stand as sulphate of soda in considerable proportion, muriate of soda and magnesia in small proportion, supercarbonate of lime, silica, and oxide of iron, or silicate of iron.

The gas which rises, Mr Phillipps found to contain five parts of carbonic acid and ninety-five of azote in the hundred parts. It is said that he ascertained, by careful experiment, that the water contains no azote. Yet it is remarkable that this gas is contained in several of the Continental springs; and even the tepid spring of Buxton contains it in good proportion.

Dr Tunstall gives, as most recent, the analysis of Mr H. Noad, lecturer on chemistry at St George's Hospital. We insert them in three columns; the first, the immediate results of analysis; the second, the calculated composition; and the third, the probable actual constitution in an imperial quart.

Results.	Grains.	Calculated Composition.	Grains.	Probable Actual Constitution.	Grains.
Lime, . .	8·3500	Sulphate of lime,	20·0500	Chloride of calcium,	5·4500
Magnesia, . .	1·3600	Chloride of sodium,	5·7200	Chloride of magnesium, . .	1·4800
Silica, . .	·7000	Carbonate of soda,	1·2000	Sulphate of soda,	6.9600
Sodium with chlorine,	2·2700	Chloride of magnesium, . .	1·4880	Carbonate of soda,	1·2000
Soda with carbonic acid, . .	·7500	Silica, . .	·7000	Sulphate of lime,	13·3100
Sulphuric acid, .	11·6800	Proto-carbonate of iron, . .	·1087	Silica, . .	·7000
Chlorine, . .	4·2000	Error, . .	·6333	Proto-carbonate of iron, . .	·1087
Proto-carbonate of iron, . .	·1087			Error, . .	·6913
Carbonic acid, .	·4813				
	29·9000		29·9000		29·9000

We acknowledge that we have extreme difficulty in believing, that the sulphuric acid exists in combination with lime to so large an amount, as to form thirteen grains in the imperial quart, and little less difficulty in believing that chloride of calcium exists, in the same quantity of water, to the extent nearly of five grains and a half. We still, therefore, are inclined to adhere to the opinion that, in the actual water, the salts are most probably arranged in the form of sulphate of soda, chloride of sodium, sulphate of magnesia, and supercarbonate of lime. Such a combination of saline materials we can believe to exert, in proper solution, and taken for a sufficient length of time, decidedly good physiological effects; and, possibly, these may be subsequently followed by therapeutic changes. On the other hand, if, as Mr Noad infers, the Bath water contains in each imperial quart so much sulphate of lime as 13·3 grains, which is twice the quantity of sulphate of soda, and more than twice the quantity of chloride of calcium, it becomes difficult, according to the usual modes of considering the action of chemical agents upon the animal body, to understand in what manner, or by what means, beneficial effects are produced.

The accuracy of the analysis and its results we do not call in question. But the inferences are conjectural, and may be not legitimately deduced. The reason for this opinion is, that the results of the analysis of watery solutions containing several different salts are influenced by the degree of dilution and concentration. From a solution containing muriate of potass and sulphate of soda, it might be imagined that sulphate of potash and muriate of soda would be formed. Only, however, if the solution be slowly boiled, is sulphate of potass obtained by crystallisation. If it be boiled rapidly, the products are a very small proportion of sulphate of potass, much muriate of potass and sulphate of soda, the latter of which can be separated from the muriate of potass by crystallisation. From this it must be inferred, that, at a certain point of concentration, a new arrangement of the elements takes place.

On the subject of diet and regimen to be observed during the use of the Bath waters, Dr Tunstall gives directions similar in many respects to those delivered by other physicians. He is perhaps more liberal than many others, in so far as he allows animal diet once daily, and permits patients to have not only breakfast and dinner, but luncheon and supper. Though he proscribes stimulants of all kinds, and especially the stronger kinds of malt liquor and the lighter sorts of wine, (?) yet he allows the use of very weak Cogniac brandy and water.

As to clothing, he is opposed to the use of flannel, and recommends in preference the mixed fabric known by the name of merino, and cotton. All bandages round the joints, upon the same principle, and also in rheumatism and gout, because they impede movement, he discards.

The season of the year most suitable for visiting Bath varies according to the disease or diseases for which their use may be recommended. The winter season Dr Tunstall represents to be best for those who wish to employ the waters for the treatment of diseases induced by debility and exhaustion; and, in those instances in which mild climate and warm air are recommended, this season is to be preferred, providing care is taken to avoid exposure to cold.

From the middle of February to the end of April, and from September to the end of November, are stated to be the best times for persons afflicted with cerebral and spinal palsies; because, during these months, the extremes of temperature are avoided, and exercise either active or passive may be employed.

For the treatment of diseases of the skin, the warmer months are most suitable; because, as the general health is not materially affected, and the object to be attained is the restoration of the cutaneous functions, free exercise in the open air forms the most powerful auxiliary in the cure. For the cure of metallic and local palsies, that of nervous diseases, and simple rheumatic affections, the Bath waters may be employed at all periods of the year. For gout and rheumatism, which vary much, both in intensity and degree, no general rules are given.

A proper course of Bath waters is supposed to extend to the space of six weeks. But the author adds, that he has seen repeated instances in which they have continued to produce benefit for four and five months. In general, if the waters agree, perceptible improvement is manifest after a few days. If the patient thus steadily improve for six weeks, it is recommended to suspend the use of the waters for a short period, and again employ them for a second course of six weeks, as more eligible than persisting in a more prolonged course. Dr Tunstall reminds his readers that the great object in the administration of all remedies requiring time for their operation is to assist nature in relieving the system from disordered action, whether functional or organic; and experience shews that the effects of the Bath waters are cumulative, and that the benefit derived is, in the majority of instances, not fully developed until two or three weeks have elapsed after they have been discontinued.

The administration of the Bath waters is external, in the form of baths and dry pumping,—and internal by drinking, with modifications suited to each form of disease.

The usual period for drinking the waters is between nine and eleven in the forenoon, and between three and four in the afternoon. This duty the patient is recommended to perform at the pump-room, if possible. The amount drunk should not exceed four ounces for the first six doses; afterwards ten ounces may be taken for eight doses; and, if the water agree, the full amount, which is from one pint to one pint and a half daily, may be continued. Moderate exercise should be taken after each dose.

The hot bath the author distinguishes as varying from 105° to 107°; the warm bath from 98° to 102°; the temperate bath at 95°; and the tepid at 86°. These rates are a little more elevated than those which physicians are elsewhere accustomed to employ, in order to distinguish the temperatures of different baths. The limit from 65° to 85° has been employed to designate the tepid bath; from 85° to 95° the warm bath; and above 95° to about 104° the hot bath. At Bath a large swimming bath, of the temperature of 86°, is kept constantly filled in the neighbourhood of the hot spring. This is four degrees higher than the temperature of the natural bath at Buxton.

The time during which the individual remains in the bath is regulated by the symptoms and the circumstances of the case, and also by the temperature at which the bath is employed. The author recommends that he should remain in it not longer than twenty minutes, though some, he adds, come out refreshed after a longer period. We think from what we have seen of cold, tepid, and hot baths, that in a bath at 86°, it will scarcely be possible for patients of all kinds to remain so long as twenty minutes. Ten minutes in such a bath we believe to be a more safe maximum; and, for not a few cases, five minutes will be the longest time that such a bath can be used. At 92°, and 95°, on the other hand, the patient will readily remain fifteen minutes.

Three baths in the week Dr Tunstall represents to be as much as the system can bear with advantage in any case. If the douche bath be used, two baths and two douches are the most eligible.

It must not be forgotten that the use of the Bath waters has a tendency to induce plethora and some vascular excitement. This therefore requires to be watched; and if the patient present symptoms indicating such excitement and congestion as may seem dangerous, their use must be suspended, and suitable remedies, dietetic and medicinal, must be administered.

The Bath waters have been recommended as highly serviceable in many morbid conditions of the human body. But if these be attentively considered, probably their number is not so great as has been usually thought. The disorders in which Dr Tunstall thinks they may, under proper management, be beneficial, are—*first*, certain nervous diseases, as chorea, lumbago, sciatica, and neuralgia; *secondly*, local and mineral palsies; *thirdly*, such chronic diseases of the skin as lepra and psoriasis, eczema and impetigo, some syphilitic eruptions; *fourthly*, chronic diseases of the uterine function, as hysteria and sterility; *fifthly*, what the author styles chronic diseases of functional origin, comprehending under this head what he denominates brain-fag, debility ensuing after febrile diseases, debility after fractures and injuries, dyspepsia and hypochondriasis, and, in the last place, arthritic diseases, including the forms of gout and rheumatism.

In chorea, or St Vitus's dance, Dr Tunstall represents the Bath waters to be useful alone. The best method of applying the waters is the warm douche, by means of a rose, the force being regulated by the age and strength of the patient, who is further recommended to drink a small glass of the waters daily. The treatment occupies from six weeks to two months, according to the previous duration of the disease. When symptoms of phlethora, inflammatory action, or determination of blood to the head ensue, the application of one or two leeches behind each ear is recommended as the most eligible form of depletion. If purgatives are required or exhibited, he recommends the examination of the motions.

The only other remedy besides the use of the waters, which he regards as worthy of mention is sulphate of zinc, in doses of one grain twice daily, gradually increased to sixteen or twenty grains, given in the form of pill, immediately after taking food; and in this case a tepid shower bath is recommended instead of the rose douche.

In the treatment of lumbago and sciatica, the author trusts not to the waters alone. Local depletion by cupping over the seat of the pain, the revellent effects of a blister, and the derivative operation of meadow saffron, with low diet and antiphlogistic remedies, are all recommended as proper in the first instance. If after these means have been tried, the patient is not relieved, then he must try the thermal treatment of the baths; and even along with this thermal treatment Dr Tunstall recommends the method already mentioned with acupuncture, galvanism, and the use of iodine and stimulant embrocations.

The bath should be employed at the usual temperature three times in the week; and when the patient can bear the pressure, it should be alternated with the wet douche, and afterwards with the dry, while the patient drinks the waters moderately for three weeks. It is when no perceptible improvement takes place in the local disease, that the other remedies external and internal are recommended.

The internal remedies most useful are quinine, iron, and iodide of potassium.

The first of these remedies he represents to be indicated, when the pains are aggravated in the evening or night, and when the disease assumes the intermittent character. The second he pronounces to be altogether hurtful, because all chalybeates tend to produce constipation, and because these medicines rather retard and impair than accelerate the beneficial effects of the Bath waters. Iodine, however, or rather the iodide of potassium, he considers to be most serviceable as an alterative and tonic medicine; this action he thinks depending upon its decomposition; the iodine being set free and acting on the nervous and glandular system, while the potash tends to neutralize the acidity which, he says, more or less attends all chronic affections. Five grains twice daily is the dose which is said to be found most beneficial to patients when using the Bath waters.

The conclusion is this, that the Bath waters are in many instances adequate to effect a cure of lumbago and sciatica; and that in other instances they appear to possess the property of increasing the sanative powers of other remedial agents.

In the majority of cases of Neuralgia the effects of the Bath waters are palliative rather than curative. These cases belong to the order in which the cause is deeply seated in the nervous system. But there is another order of cases of severe nervous pain, which have not, according to the author, a constitutional origin, instances of which he names Simple Neuralgia; and in these the greatest advantage is derived from the use of the Bath waters, providing these waters are employed before the disease has become habitual. This is equivalent to saying, that in slight cases, the Bath waters are, like other remedies, beneficial, and in severe cases their effects are, like other remedies, only palliative and temporary in operation.

In one form of *Neuralgia*, to which the author applies the distinctive denomination of Mercurial Neuralgia of the Joints, he represents the use of the Bath waters administered in the proper manner to be most efficacious. As an example of this disease and the beneficial effects of the waters upon it, the author gives the following instance as having taken place in his own person.

"In July 1838, while walking in London, I was knocked down by a truck, falling heavily to the ground. I bruised my right knee-joint, which subsequently inflamed and involved the inguinal glands. I was at once ordered to bed; leeches and other antiphlogistic remedies were had recourse to; but still chronic tumefaction remained both in the joint itself and in the glands above, for the relief of which blue pill internally, and the application of mercurial ointment were directed. This treatment was continued for three weeks without any effect upon the gums, but with great relief to the parts affected, when, one afternoon, just as I had got out of bed, I fell fainting upon the floor. Reaction taking place, I experienced severe pain in the neighbourhood of the hip-joint, with inability to use the slightest exertion without extreme agony. I became perfectly crippled upon the right limb, which wasted away until I could nearly grasp the right thigh with the right hand. Constitutional disturbance set in; I had night perspirations and all the symptoms of rapid decline, with the exception of cough. I consulted many eminent men without relief, until Sir Richard Dobson, of Greenwich Hospital, suggested the Bath waters, and I came to Bath more dead than alive, at the latter end of October. My friend, Dr Garrett Dillon, suggested that I should see the late lamented Dr Barlow, whom I so much honour both as a kind mentor and as an instructor in the peculiar diseases relieved by the Bath waters,—whose name I cannot mention without a sigh for his loss,—whose precepts have been my guide in many a season of difficulty and trial,—and whose practical knowledge and experience were always at my command.

"Dr Barlow put me upon a plan of treatment, ordered me to drink the waters, and to bathe thrice a-week. In three weeks I threw away my stick, in six weeks I danced a quadrille, and in two months I was so completely restored to health, that my limb resumed its natural size, and regained its former strength; and I have never experienced lameness, ache, nor pain in the limb since that period."—Pp. 41–42.

The diagnostic sign of this form of articular disease, Dr Tunstall represents to be, that the limb can be made to go through all its natural motions, if done gently, without pain; but that the patient himself is unable to perform one of these motions, and that pressure with the fingers and pressure with the whole hand, produce a nearly equal amount of pain; that this pain nevertheless occupies too large a surface to indicate disease of the joint itself which is implicated in the neuralgic affection; that the disease is one of debility without features of inflammation or excitement; in short, of innervation.

Local palsies, the effect of mineral poisons, especially of lead, and occasionally, as the author thinks, of copper and other minerals, are known in Bath under the name of dropped hands and dropped feet. Persons suffering under this disorder, which has generally been preceded by attacks of colic or intestinal pain, are cured by the Bath waters, if they resort to their use early; and even though the cure may not be perfected at first, it gradually becomes complete under the influence of air and exercise, and continues until the metal again exerts its influence.

There are certain examples of local palsy, however, in which either the action of mineral poison is not known, or escapes notice; and in these the disease being referred to the state of the brain, according to Dr Tunstall, the patient is treated by general blood-letting, the use of setons, blisters, and issues, which, instead of producing benefit, increase the local palsy by weakening the general nervous system, and impairing the health. Many cases of this order consequently, which, had they been sent in the first instance, would undoubtedly have been cured, do not derive any benefit on account of the severity of the remedies which had been employed, in order to relieve a diseased condition which had no real existence.

In the case of palsy from lead, independent of the colic by which that disorder is very constantly preceded, the blue or leaden coloured line on the gums will always serve as a good diagnostic mark. Painter's palsy is often the consequence of the presence of an extremely small quantity of lead in the system. When the disease appears in persons in the upper and middle ranks, it is most frequently the consequence of the daily use of water conveyed in leaden pipes to leaden cisterns. In another class of persons, it may sometimes, according to Dr Tunstall, be traced to the practice of drinking, while at work, malt liquor brought in a pewter-pot, and which, by standing a certain time, sometimes in the pot to be warmed, becomes impregnated in this manner with a portion of lead, sufficient to induce colic or palsy, or both. The acid of the liquor, in this condition, forms from the pewter a salt of lead, which being drank by susceptible persons produces its specific effects.

In other instances, local palsies appear to be the result of excessive exertion of the muscular parts. For instance, the fingers and wrists in teachers, and practitioners upon the piano and organ; writers, from the labour of using the pen, are all liable to paralytic attacks of the hands and fingers, and in the first case, of the fore-arm, and apparently in consequence of the excessive labour to which the muscles and tendons are subjected.

"Modellers of cornices in wet plaster,—those who level stone by the application of sand and cold water,—quarrymen who have to prepare wet slabs of freestone,—those who receive the damp sheets of paper from the printing-press for folding,—are all liable to this species of palsy; and I have seen cases in which it could not only be referred to one or other of these causes, so that I feel called upon to say that, where the palsy commences with both hands, we are bound to regard it either as the effect of a mineral absorbed into the system, or directly applied to the parts affected, or as a result of mechanical pressure, or of the continued application of cold to the extremities of the nerves supplying the parts implicated in the paralytic condition; in other words, that it is the effect of a cause which requires the medical practitioner to inquire into the avocation or amusement of the patient who places himself under his professional care, in all cases where loss of power over a set of muscles occurs without direct evidence of cerebral disturbance."—P. 50.

Similar conditions of local palsy are liable to affect the feet.

In all these instances of local palsy, Dr Tunstall represents the employment of depletory measures to be hurtful, and those of derivation and revulsion, or counter-irritation, to be doubtful, if not useless; and he regards the Bath waters, properly used, as the one sovereign remedy.

"In these diseased conditions of the nerves, the Bath waters seem to act miraculously. They expose the whole surface of the body to the direct action of the tonic properties of iron, and, administered internally, the system, as it were, is saturated with the antidote to mineral poisons. By increasing the volume and power of the pulse, diminished nervous energy is changed into excited power; the constitution, previously reduced in muscular fibre by the long impregnation of a deleterious and subtle poisonous principle, is, as it were, restored to vigour; and the cure may be observed to progress daily. Such, indeed, is no exaggerated sketch of the effects of the Bath waters in mineral palsies; and I have never, in any one instance, observed them produce any ill effects where they have been judiciously persevered in even for a longer than ordinary period in those cases in which no constitutional predisposition to disease existed.

"In the more obscure cases their results are equally beneficial. When early submitted to their effects, it is manifest that when the constitution has been subjected to violent remedies, and the nerves have remained as it were useless for a long period, the cure will be protracted, and the benefits derivable from the thermal treatment somewhat doubtful. Nevertheless, although the special complaints may but slowly improve, as long as the system gains rather than loses by the employment of the waters, their use is to be persevered in."—Pp. 53–54.

Dr Tunstall allows, however, that the therapeutic influence of the Bath waters may be materially aided by the internal use of iron, the external aid of galvanism, both within and out of the bath, and the employment of stimulating embrocations and blisters. The latter he recommends in the painter's palsy, and after its application the use of Pemberton's splint.

The early employment of means to excite the nervous and muscular energy he strongly recommends, and is opposed to the use of measures which interfere with this. Thus, as soon as the patient can grasp any object, he recommends the use of the dumb-bell, or attempting to raise a weight by means of a pulley. The use of crutches, on the other hand, he condemns.

In the removal of scaly diseases of the skin,—for instance, leprosy, psoriasis, and pityriasis,—Dr Tunstall represents the Bath waters to be peculiarly well suited. These diseases, which, he observes, are often attempted to be treated by means of mercurial and arsenical preparations, and often resist these means, even though carried the length of impairing the general health, give way in no long time to the use of the Bath waters. If this be the fact, why should not baths made in imitation of the Bath waters produce equally beneficial effects?

Dr Tunstall thinks that the minute nosological distinctions of cutaneous scaly disorders are of little or no use; because, he argues, these forms pass into each other, and even into pustular diseases.

He mentions the case of a female whom he saw covered from head to foot with inveterate *psoriasis*, in which the body discharged daily an enormous amount of scales, bearing a proportionable weight to the quantity of the cutaneous exhalation of a healthy person. This case was completely cured by the application of Barbadoes naphtha, by means of a painter's brush, which prevented the formation of scales, and by the use of a concurrent three months' course of Bath waters. This patient, however, had *scabies* previous to the appearance of *psoriasis;* and as soon as the latter was cured, *scabies*, in a severe form, appeared, and affected more than one of the attendants who had constantly waited upon the patient, though they were unaware of its existence during the treatment of the scaly disease.

All cutaneous diseases, in short, Dr Tunstall is disposed to ascribe to general morbific causes, which act probably in deranging the circulation; as cold, want of cleanliness, errors in diet; and the occurrence of any particular eruption he imputes to habit, temperament, and idiosyncrasy. This doctrine, it may be observed, is not new. It has been long taught, in different forms, by different authors. The great objection to it,—the greatest defect with which it is chargeable, is, that it is vague, and very far from being precise. Probably the subject admits not of greater precision. At all events, it is clear that in the present volume the doctrine has not been rendered more precise.

The operation of cold, which it is not difficult to understand in a general manner, is represented to be something of the following nature:—The exhalations are first, in the production of squamous diseases, entirely suppressed; the follicles then become enlarged, one after another; and the leprous patch is the result, enlarging from within outwards. When the part is to get well, healing commences with those follicles which first assumed the diseased action, proceeding from within outwards, and is completed by the gradual absorption of each individual cell into a healthy state. These follicles or cells, in their diseased state, secrete matter which solidifies upon reaching the surface, and which constitutes the varieties of lepra and some forms of *psoriasis;* and the cure does not commence until moisture exudes from some portion of the scaly patches, that is, until the cutaneous function is restored.

Such is the theory of the nature and mode of cure of two important cutaneous diseases, according to Dr Tunstall. It would not be difficult, did space and time permit, to point out several defects in this theory. But our duty is rather to present than to criticise opinions and doctrines.

The same theory he applies to explain the phenomena of the *psoriasis*, or *eczema*, or *erythema*, as it may be, in dyspeptic and arthritic persons. The author next states, that among the diseases of the skin, which being squamous in character, in most instances arise from accidental or long continued errors in diet, *herpes* and *eczema* may be cited as instances. Now the truth is, that neither herpes nor eczema are scaly diseases, unless the author proposes to break down more completely than has been hitherto done, all distinctions, both etiological and nomenclatural. Herpes and eczema are vesicular diseases. The former is an affection comparatively acute, and runs its course in general in a few days, requiring only low diet, laxative and antacid medicine, and regulated diet afterwards. The latter is a little more chronic, and certainly causes, or is attended with more cutaneous irritation and suffering. These forms of disease the author states, that a well regulated course of Bath waters, with strict attention to diet, rarely fails to cure. It appears to us that Dr Tunstall employs the denominations herpes and eczema in a manner quite different, from that in which they are commonly meant. Both diseases, unless in some exceptional cases, for instance *herpes iris*, are commonly easily enough managed by the use of the means already mentioned; and the use of Bath waters must in general be entirely supererogatory.

Dandriff or pityriasis he next represents to pass occasionally or be converted into *tinea;* that is, according to our understanding, into *porrigo*. But even in this aggravated form, it gives way to the influence of the Bath waters. Now we neither altogether deny the inference, or assumption rather, adopted by Dr Tunstall, that it is possible that different and essentially distinct cutaneous diseases may spring from the same remote causes; but we must confess our inability to discover, that this is a reason for confounding together affections of the skin, which, by skilful and experienced dermatologists, have been hitherto distinguished from each other. It would be very desirable, that, upon this subject, the author would peruse the observations of Malmsten, published in this Journal, volume seventy-third, and the previous observations of Plumbe, and he would then see that the disease called dandriff is altogether different from *Tinea* or *Porrigo.* It has been hitherto very generally taught, and believed to be taught upon good grounds, that *Porrigo* or *Tinea*, as the author calls it, is a disease affecting the roots of the hair, the piliparous sacs or follicles; while dandriff, or *Pityriasis*, as we have understood, is rather an affection of the outer surface of the true skin, and consequently causing the formation of diseased cuticle in the form of branny scales. At all events, it is not known to us that dandriff has been shewn to arise from affection of the follicles and sacs, and roots of the hair.

The circumstance now mentioned is very likely to impair somewhat the confidence of readers in the writer's diagnosis, and consequently in the adaptation of treatment; for if diseases different from each other are in this manner confounded, it follows that any results of experience in the use of remedies, and especially the remedy the efficacy of which the author advocates, must be received with doubt, and may be of greatly less value than otherwise they would be.

Dr Tunstall very properly, however, directs attention in the treatment of *lepra* and *psoriasis*, and, if we do not misunderstand him, porrigo, to the state of the digestive function, and the condition of the teeth in the instance of children. It is, nevertheless, a considerable drawback, that here also the want of distinction renders his instructions less explicit than is desirable. Thus, he informs the reader, that "Dyspepsia, with the lithic acid diathesis, will almost invariably be found as a prominent constitutional symptom." Yet he does not specify the exact cutaneous affections in which this takes place, further than by placing this remark under the head of *Lepra* and *Psoriasis*, while he is at the same time speaking of diseases of the scalp.

In the treatment of the latter affection,—porrigo we presume,—he mentions in favourable terms the use of petroleum soap, which is a mechanical mixture of Barbadoes tar with soap.

Among remedies for amending the state of the alimentary canal and its functions, he mentions the compound mixture of Chirayta (Gentiana Chirayta,* *Agathotes chirayta*) as one of great utility. As an alterative, he recommends

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* This term is one which is perfectly mystified by the different modes in which it is written and spelled. It is sometimes written *Chiretta, Chiraita,* Chirayta, and Chireta; and, to improve the matter, Dr Tunstall writes it *Charyita*, a method certainly altogether unsanctioned by any authority. It is

Plummer's pill without the calomel; that is a pill formed of sulphuret of antimony, which he says is the best part of the compound, regarding the calomel as hurtful. One or two of these antimonial pills may be taken every night, and during the day two fifteen minim doses of antimonial wine. During the antimonial course, the best period for bathing is the latter part of the day; and it is recommended that the patient, upon leaving the bath, should be packed in blankets for twenty minutes at least, in order that the cutaneous function may be encouraged, and that afterwards he should return home speedily, that the action of the skin may not be checked. To this mode of management, and confining the patient within doors during the rest of the day after bathing, which is the practice observed in the Bath Hospital, the author is inclined to ascribe much of the benefit which follows the use of the waters.

Of arsenic as an adjuvant, he speaks not very favourably. The use of this mineral is known to exert an irritative action upon the mucous surfaces, causing influenza and analogous symptoms. The Bath waters, according to his account, modify the action of the mineral; and doses of from three to five minims of Fowler's solution seem to act more powerfully than larger quantities. The deduction from this fact is, that the exhibition of arsenic in combination with the Bath waters requires close and assiduous watching; and that by their united agency cases have been cured which have resisted their separate employment.

We have more than once directed attention to the fact, that the best mode of administering arsenical medicines is to unite with them the use of chalybeates.

Dulcamara and sarsaparilla he pronounces to be of no use.

Dr Tunstall also disapproves of the use of ointments containing animal grease. Cerates he thinks more serviceable or less hurtful. But he expresses the opinion, that when instances of cutaneous disease get well, apparently under the use of these applications, while the Bath waters were also in employment, the latter effected the cure in spite of, rather than in consequence of, these remedies. In short, when the patient with leprosy or psoriasis, or any other cutaneous disease, we suppose, repairs to Bath, he must confine his attention solely to the use of the waters, and discard all external local applications, excepting those which merely soothe the irritated membrane. Among these he mentions cerates, com-

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desirable that one uniform method of writing the term, which is Indian, should be adopted and adhered to. The one which is the nearest to correct, is that given in the text, *Chirayta*, which is the same as that given in Lindley's Medical Flora, the works of Dr Pereira, Dr Royle, and other works of good authority. In the Bengalee it is *Chirata*.

posed of the oxide of zinc, magistery of bismuth, carbonate of magnesia, Barbadoes tar, nitrate of mercury, nitric oxide of mercury, iodine, and glycerine.

The eruptions ascribed to syphilis, which the author denominates tertiary, he represents to be obviously amended under the external and internal use of the Bath waters. The wandering nocturnal pains are relieved, the disposition to periostitis is checked, and the impaired health is re-established. In no form of general cachexia, he adds, do these waters produce a greater amount of benefit.

Such is the evidence of Dr Tunstall. It must not be omitted here to say, that if the author had informed us in what number or proportion of these cases of alleged tertiary syphilitic eruption and periostial pains mercury had previously been given and taken, he would have rendered his information greatly more serviceable than it is. It is scarcely necessary in the present day to say, that we entertain doubts as to these eruptions actually depending upon the operation of syphilitic poison, and are confident that the periostial pains, nodes, and other indications of periostial disease, do not depend on the action of this poison, but on the deleterious influence of mercury ; and it is accordingly our opinion that, in any estimate of the therapeutic efficacy of these waters, this distinction ought to have been kept in view.

Of late we have had occasion to direct attention to the etiology of those diseases of the female, which are believed to depend on morbid conditions of the organs of reproduction, and to the hurtful effects which these diseases produce on the general health. Whether these diseased states are marked by amenorrhœa, dysmenorrhœa, chlorotic symptoms, hysteria, or hysteric neuralgia, in all forms, Dr Tunstall represents the Bath waters, if properly administered, to be most efficient and beneficial in operation. Patients brought to Bath in the most helpless state, from nervous debility and irregular determination of blood, with pain generally in some part of the spine, and the various symptoms indicating the disordered state of the ovarian influence and the uterine secretions, are after a certain time, not only delivered from their painful feelings, but become strong and comparatively vigorous.

In those severe forms of the disorder, in which palsy of the lower extremities is superadded, the author properly dissuades from the employment of depletion. Indeed, all judicious observers are now aware, that in cases of this class, depletion fails to produce the benefit expected to result from it. There is not inflammation, but, he says, increased sensibility. This is true; but sometimes the rachidial veins are loaded with dark-coloured unrespired blood, blood which is often contaminated if not poisoned by the presence of hurtful elements, which require to be eliminated. Even cathartics, in these cases, he pronounces to be hurtful, if used much and indiscriminately. The enema answers best for the purpose of keeping the lower intestines in a less distended state, and restoring to the whole alimentary canal its proper functions.

The main remedy, however, is the use of the Bath waters, externally and internally. The patient should begin first with the bath at 98°, taken two or three times every week, according to the judgment of the medical attendant. Friction, by means of an attendant, should be made after the use of the bath, while stimulating liniments and sinapisms may be employed over the skin of the back. As soon as possible, the patient should be taken in a wheel-chair to the pump-room, for the purpose of drinking the waters.

Next comes the douche, moderately increased in number and force, and then alternately the tepid shower-bath, for a period of six or eight weeks.

The period usually allowed for a full course of the waters in cases of this class, varies from ten to twelve weeks, with remissions at the end of every fourth week; and should the periodic uterine secretion return, their use must be suspended during its presence, both in this and in other disorders.

In sterility the Bath waters are represented to be equally beneficial.

The overworked brain, or that state of the cerebral circulation which takes place in consequence of excessive and unremitting mental labour (*Enkephalopathia Literatorum*), is another condition in which Dr Tunstall represents the Bath waters to be highly curative. The subjects of this complaint are well known. Authors, writers in periodical publications and newspapers, solicitors and solicitors' clerks, clerks in mercantile houses with large business, clerks and writers to engineers;—all these classes of persons have been known to suffer in different degrees from the overtasked labour of the brain, and its circulation. Of course, besides the indications of disorder in this organ, as irritability, restlessness, sleeplessness, disagreeable dreams, headach, ringing in the ears, giddiness, palpitation, irregular and intermittent pulse, dyspeptic symptoms, and indications of torpor of the hepatic and renal secretions, are present. In cases more severe and perhaps farther advanced, *Paraplegia* comes on, or at least what is called *Paresis*, a peculiar paralytic affection of the lower extremities, indicated by unsteadiness in walking and a feeling of insecurity. In several cases the symptoms are so severe as to imitate those of softening of the brain. From this, however, those of the over-excited brain are to be distinguished, according to the author, by two circumstances. The first is that the intellect is clear; the second that the state of the pulse indicates debility of the circulating system. It should not be forgotten, nevertheless, that this state of matters does in certain instances proceed to affect the structure of the brain.

For all this the grand remedy, according to Dr Tunstall, is the use of the Bath waters. We may be permitted to remark, nevertheless, that though we entertain no doubt of the utility of Bath waters for many sufferers in the manner above mentioned, yet a small amount of reflection and common sense is sufficient to convince any one, that the most effectual remedy where it is practicable, is relaxation, rest from labour, in the country amidst fresh air, a vacation in short from the incessant labour of the mind, the eyes and the fingers. Residence in the country, especially by the sea-coast, is the resource which has appeared to us to be most effectual. In other instances residence in a mountainous district, as Westmoreland, Wales, the Highlands of Scotland, or Switzerland, has accomplished wonders in the removal of the symptoms of this condition of the system.

The great difficulty in dealing with a considerable proportion of the cases coming under this head, is, that in situations in which the chief remedy, rest and relaxation from mental labour, is urgently required, the circumstances of the individual scarcely admit of it, without such detriment to his worldly and social prospects as amounts almost to present ruin. The disorder rarely takes place in persons, in what are called easy circumstances, for the obvious reason, that persons in that state seldom exert the mental faculties to any great degree, and certainly only in exceptional cases to the degree of overworking the brain. Most commonly, on the other hand, it is in the hard-working and underpaid literary or professional drudge, who is obliged, in order to obtain subsistence, and maintain his social position, however insignificant, to exert himself without intermission the whole round of the year, and often to. do so, not only during the day, but far into the hours of night and morning, that this complaint is seen in well-marked characters and considerable degree. The prescription of sending such a person to any situation away from the scene of his làbours, is equivalent to inflicting the loss of income for the time and perhaps for a long time subsequently. If the use of the Bath waters could diminish even this part of the evil, they would confer a great and effectual benefit on many of the patients of this order. But this we are assured they cannot do; and the inconvenience and evil must remain, so far as they are concerned, unabated and unmitigated. On this subject, however, which is at once delicate and not agreeable, it is unnecessary to dwell. It seems as if, in the incessant struggle either for subsistence or competence, a certain proportion of the competitors necessarily sacrifice that which alone makes existence endurable.

The debility occasionally succeeding to febrile diseases is mentioned as a state, for the cure of which the Bath waters may be confidently employed. In this instance there is strong reason to believe that the waters exert no specific effect, and that the benefit results from the change of air and scene. So confident, however, is the author of the justice of the recommendation, that he informs the reader that, from this circumstance, Bath derived the Saxon appellation of Aikman Chester, the City of the Sick Man. Most Anglo-Saxon scholars are of opinion that *Aikman* is Oakman, *Aik* being the Anglo-Saxon denomination for the Oak, and still represented in modern German by the analogous term, *Eich*, and in English by the compound derivative, Acorn; *Aikorn; Eichkorn.* It may be doubted whether the author excels in etymology.

The therapeutic directions for the management of dyspepsia and hypochondriasis are good. The author advises that the patient should trust most to restricted diet, and avoid medicine, always excepting the Bath waters. We have merely to observe, that in the treatment of both these diseases, bathing, both warm, hot, and cold, are too much neglected, and that often, without aid from medicine, their influence upon the cutaneous circulation would be sufficient to remove the disease.

Gout and rheumatism, Dr Tunstall thinks, ought to be considered in their acute stages, as varieties of the same morbid condition, produced by different causes; in the former by errors of diet long continued; in the latter caused most commonly by cold; while under the last-mentioned agency, with hereditary predisposition to gout, the variety denominated rheumatic gout is produced.

While temperance has rendered gout a less frequent disease, he allows that what is called rheumatic gout is still common, and that it is by no means rare among the poor and hard-working classes of society, in both sexes, and at all ages. The author disavows drawing nice distinctions between gout and rheumatism. These disorders he views in their chronic form as modifications of diseased action in the joints, with general derangement of the system; their prominent symptoms being dyspepsia, pain, and swelling of the joints, without redness, degeneration more or less complete of the articulations, with contraction and rigidity of the fibrous structures, all arising from a previous inflammatory condition.

Having established this principle of the inflammatory nature of both disorders, he impresses the necessity of a modified antiphlogistic system, both therapeutic and dietetic, in order to oppose their progress, and counteract their effects. These disorders, namely gout, rheumatism, and rheumatic gout, he infers, require to be treated upon the general principles which regulate the management of all inflammatory conditions. The opposite extremes of never practising venesection and depletion, and carrying these evacuations to an extreme, are equally to be avoided. Small venesections are more advantageous than large ones, and local bleeding is often useful when general depletion is injurious. This, with the use of calomel, meadow-saffron, wine, or calomel and opium, are at the outset the most eligible measures. The object and purpose of these measures is to break, as it were, the force, and abate the intensity of the inflammatory part of the disorder; and in general cases treated in this manner more easily and speedily admit of the efficacious employment of the Bath thermal treatment.

When the palliative and soothing or inert method of treatment has been employed, it leaves after it much stiffness and immobility, and sometimes lameness and deformity. The medical attendants in the Bath Hospital, he remarks, can point out, at first sight, the cases in which venesection has been, and those in which it has not been employed. The distinctive characters are indicated by the severe distortions which are left after the soothing treatment; the worst cases admitted being those, in which the disease has been treated in its acute stage by means of opiates and meadow-saffron, too long continued. This method, with prolonged confinement in warm apartments, the use of stimulants and full diet, he regards as most hurtful; and maintains that temperance, fresh air, and moderate exercise, are the requisites next to skilful medical treatment for such cases, in order to avert stiffness, and the loss of the use of the limbs.

After medical treatment has been duly tried, then if the disease abate not, and the patient recover not strength and health, the Bath waters present the next remedy.

"As a general rule, patients are not recommended to resort to Bath until considerable immobility of the joints has taken place. It were well that they were sent thither as soon as all acute symptoms had subsided; then, when the system is endeavouring to free itself from the over-carbonized condition of blood, when the joints are enlarged by an undue secretion, and the stomach labours under dyspepsia, causing the kidneys to secrete an abnormal fluid, the thermal treatment is most desirable, the restoration to health most complete." —(Pp. 102, 103.)

In accordance with the principles above proposed, Dr Tunstall strongly disapproves of long-continued indulgence and rest to rheumatic limbs, and he recommends not only motion, but perseverance in moving the limbs, and resistance to the motionless and indolent state.

> "I have seen hundreds of cases wherein the limbs, from being confined entirely to one position, and moved, as it were, without the motion of the several joints, have become disorganized in their several articulations, and nearly immovable, which have been greatly restored by a steady perseverance in a course of the Bath thermal waters, with the auxiliary agents, galvanism, friction, iodine, and other means hereafter to be enumerated.
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> "The treatment of chronic rheumatic affections, including gout, requires—more than perhaps any other class of morbid actions—the employment of moral as well as medical means. The patient must be encouraged to exertion, to prevent immobility; he must be kindly but forcibly impressed with its absolute necessity: he must be told that partial motion is better than immobility, and that it is only by exercise that the synovial fluid expels the secretions which have usurped its habitation. He appeals to our sympathies, and declares that even passive motion is painful; but we must tell him that he has become crippled because his limbs were, from his long confinement, rendered painful upon the first commencement of activity; that "*vires acquerit eundo*" must be his motto if ever he intends to regain healthy power."—(Pp. 106, 107.)

It must be understood that all these efforts at moving crippled limbs ought to be made under the superintendence of the physician.

Upon similar principles he condemns the indiscriminate and excessive use of opium and opiates in the treatment of rheumatism and rheumatic gout. It must be allowed that it is scarcely possible to name anything which, in these affections, does so much harm as opium. In seeming to abate pain, it only masks the disease, often protracts its duration, locks up the secretions of the liver, and not unfrequently contributes to increase the stiffness and immobility of the joints. In some unfortunate cases even, as in that of the late S. Taylor Coleridge, the drug, taken for some time for the relief of rheumatism, has led to the practice of taking it habitually, and thereby impairing the energy of the nervous system.

As an introduction to the employment of the Bath waters in the treatment of these forms of disease, the author makes the following remarks.

> "Gout and rheumatism, however much they may differ in their causes, localization, and symptoms during the primary and acute stages, imperceptibly approach each other when they become chronic; in fact, it would, in many cases, be extremely difficult to form a correct diagnosis without the aid of chemistry. Cases which, from the mere inspection of the smaller joints, we should at once pronounce gout, are accompanied by enlargements of the knees and stiffness of the shoulders. On the other hand, we meet with cases in which the disease having, in its outset, attacked the larger joints, has gradually involved the minor ones. A good diagnostic mark is the active attack which, in gout, commences in the smaller, and in rheumatism, the larger articulations; where it is general, the term rheumatic gout may be safely employed to indicate the disease. An attack of gout is a common occurrence in those who have previously had acute rheumatism; an attack of rheumatism not unfrequent in those subject or hereditarily predisposed to gout. It is these complications and substitutions, if I may be allowed the term, of the one disease for the other, that have thrown so much difficulty in the way of a true pathological distinction between two similar diseases.
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> "It is in these cases that the Bath waters produce a vast amount of benefit. They surpass any other combination of remedial means in the speed with which they restore impaired function to the cuticular and digestive organs. Their use, however, requires to be very closely watched, and to be modified occasionally, and now and then suspended for a short period."—(Pp. 112, 113.)

The operation of the waters is detergent in cleansing and stimulating the skin, excitant in rousing the action of the capillaries and augmenting perspiration, and tonic in increasing the volume and strength of the pulse, and probably contributing to distribute more equally the blood through the several divisions of the capillary system. The agents in accomplishing these results are not only the saline ingredients, he suggests, but the carbonic acid and the iron in the Bath waters.

In employing the waters, he recommends that, upon their use alone, without other auxiliary means, reliance should at first be placed. If, alone, they do not produce the benefit expected, all that should be conjoined is the use of a mild aperient pill, and solution of iodide of potassium, with the endermic application of tincture of iodine.

In hemiplegia, we can see from what is here stated, that, as might have been conjectured, the use of the Bath waters is ambiguous, if not hurtful.

In paraplegia, depending on disease of the spinal cord, they are equally unavailing.

As to diabetes, of six cases in the course of seven years, in the Bath Hospital, subjected to the influence of the thermal treatment, one was cured, two greatly relieved, and in three no benefit was produced.

For about four or five years past, a combination of thermal and galvanic treatment has been in use chiefly under the superintendence of Mr Tylee,* medical electrician in the city of Bath. The method consists in causing the bath to form part of the electrical circuit. The patient being placed in the bath, a copper wire-brush, connected with the positive pole of the battery, is approximated by an attendant to the parts affected, and the intensity of the current regulated by the proximity of the brush. This, which is in short administering galvanism in the bath, is represented to be much milder and more easily borne than the common mode by violent and repeated shocks, as the flow is continuous and more gentle. It is said also to have the effect of calling into action a great number of minute nerves without exciting painful feelings, and to invigorate the patient in a greater degree than either galvanism without the bath or the bath without galvanism.

The author speaks with confidence of its applicability and its beneficial effects in rheumatism, rheumatic debility, and even in certain forms of paralysis.

In the foregoing account of the volume of Dr Tunstall, it has been our object to give a just view of the therapeutic efficacy of the Bath waters, and the cases most suited for their employment. It is scarcely necessary to say that we think these waters, under judicious management, serviceable as remedial agents, which have been for a long time unduly neglected. We entertain serious doubts, nevertheless, whether it be practicable to restore to Bath the confidence, which the mineral-water-frequenting portions of the community have been so long bestowing on the continental springs. But we hesitate not to say, that, to invalids who are not able to encounter the risk, impediments, and discomforts of continental travelling, and who may be unwilling or may feel it inconvenient to go far from home, Bath presents many real advantages and some comforts, for which the infirm may look in vain in more distant localities. If any desire to see all the other advantages of Bath and its vicinity as a residence fully stated, this he will find done by Dr Tunstall in a concluding chapter. They consist in mild climate, sheltered situation, cheapness, it is stated, of house-rent and the expenses of living generally. Is the latter promise indorsed by the parties most concerned?

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* Practical Observations on Galvanism, Electricity, and Electro-Magnetism, as employed in the cure of disease, with remarks on the advantages of their application through the medium of Baths; containing full directions relative to the choice and management of apparatus. By John Palmer Tylee. Bath, 1848.

This book is rendered unsightly by several errors in orthography. The instance of *chirayta* has already been mentioned. At pages 74 and 96 is dyathesis instead of diathesis; at page 90, Hahnman instead of Hahnemann; and at page 120, metastisis instead of metastasis.

ART. II.—1. *A Practical Treatise on the Diseases Peculiar to Women. Illustrated by cases derived from Hospital and Private Practice.* By SAMUEL ASHWELL, M.D., Member of the Royal College of Physicians, London; and late Obstetric Physician and Lecturer to Guy's Hospital. Third Edition, London, 1848. 8vo, pp. 772.

2. *On Tumours of the Uterus and its Appendages. (Jacksonian Prize Dissertation).* By THOMAS SAFFORD LEE, M.R.C.S.E., Fellow of the Medico-Chirurgical Society; formerly House-Surgeon to University College Hospital, and to the Hospital of Women, Red Lion Square; Member of the Royal Medical Society of Edinburgh, &c., &c. London, 1847. 8vo, pp. 274.

The last occasion on which we directed the attention of the readers of this Journal to the subject of the organic diseases of the uterus and its appendages, was in January 1844, in volume sixty-first, when the work of Dr Lever was the subject of consideration. Since that time eight years and six months have elapsed, and in one department at least, that of Ovarian Disease, much has been done, and certain experience has been acquired.

The second part of the work of Dr Ashwell is devoted to the organic diseases of the internal and external organs of the female; and as this division of the subject is now to come under review, it is proper to conjoin at the same time the treatise of Mr Safford Lee, as one in which is given an elaborate and instructive history of the tumours which are incident to the uterus, the ovaries and the Fallopian tubes.

The treatise of Mr Safford Lee, which corresponds to the second part of the work of Dr Ashwell, consists of three parts or divisions, and an Appendix.

The first part, which is devoted to the history of the Tumours of the Uterus, is subdivided into five chapters :—I. On Tumours of the Uterus; their pathology, symptoms, diagnosis, and treatment. II. On Polypoid Tumours of the Uterus. III. On Soft Polypi of the Uterus. IV. Cauliflower Excrescence of the *Os Uteri;* and V. Encephaloid Polypi.

The second part, in which the author treats of the diseases of the Ovary, consists of two chapters; one devoted to the extensive and complicated subject of Diseases of the Ovary; the other to the Malignant Diseases or Heterologous Growths incident to the Ovary.

In the third part, on the Tumours of the Vagina, and the External Organs of Generation, the author treats under two sections or subdivisions of these subjects.

The Appendix consists of five Tables, shewing the results of all the Operations for Excision of the Ovary, from 1809 to 1846, and the different modes in which the operation has been performed, and attempted to be performed.

Without attempting to enter into the details given in each work, we shall attempt to combine the most important and useful points of information conveyed in both.

An organic disease Dr Ashwell defines to be one, in which, unlike a functional disorder, the deviations from healthy action are indicated by symptoms during life and by appearances after death, which are always the result of some, and often of very considerable change in structure. Probably it might have been said, that an organic disease is that, in which one or more of the tissues have undergone a perceptible and visible change in structure, whether that change has or has not given rise to the symptoms observed during life. As examples, the products of chronic inflammation, the deposits of scrofula, and the growth of carcinoma are adduced.

Organic diseases the author represents to be less complicated and perplexing than the dynamic. Their locality is in general more easily discovered, and their extent is generally defined without much difficulty. The exact pathological nature, however, of an organic enlargement, whether it consist in excess of nutrition, merely increasing the bulk of the part; or whether the increased size and altered texture be the product of a heterologous change in the organ or organs, are questions more difficult to be solved, and about which able observers, at least for some time, often differ in opinion. The author thinks that the resemblance between organic lesions comparatively inert, and those which are malignant, is frequently so close as to render distinction a matter not easy. The diagnosis of such lesions, where it is practicable, is of great moment.

While he remarks, that we can rarely cure those morbid enlargements and alterations, he might have said that hitherto no cure for them is known. Pathology, however, has so well illustrated the nature of many, that the practitioner may avoid indiscriminate and injurious treatment. Several of these growths, although they present formidable and threatening appearances, yet admit of a certain amount of alleviation by means of rest, self-denial, and dietetic management; and some may be thus brought to what may be termed an inert and latent condition, and allow the life of the patient to be prolonged with a certain degree of comfort, up to and even beyond the usual period. In the instance of those diseases for which hitherto neither medicine nor surgery has provided any satisfactory method of treatment, both science and experience concur in impressing the precept, that the less that is done, it is better for the patient; and that everything hitherto proposed and attempted, seems to have had no other effect than that of hastening the approach of the termination by death.

It is for this reason that so near an approximation as may be possible to correct diagnosis becomes important; and this importance is increased in those circumstances, in which the diseased change is either complicated with pregnancy, or liable to be mistaken for that condition. In the way of obtaining this knowledge, and applying it in individual cases, every one must admit, that the difficulties are neither few nor small. Dr Ashwell allows, that it may be sometimes impossible to arrive at certainty regarding the precise character of complicated diseases of the womb; but he adds, that often they are overlooked or misunderstood from the want of a sufficiently early and careful examination. Not unfrequently they are misunderstood from carelessness, ignorance, rashness, and that selfish confidence which practitioners assume, for the purpose of impressing patients with the idea of their superior skill and extensive experience. Another cause which leads to the same result is the desire to do something, the habit of professing by means of peculiar methods of treatment to cure that, which may not admit of cure in the manner and by the means proposed, or which may not admit of cure in any manner or by any means.

On the other hand, it must be allowed that the exact diagnosis of diseases of the uterus, and the determination of the two points whether they are organic or not, and what may be the nature of the disorganization, is beset with peculiar difficulties. The concealed situation of the uterus, and its local relations, renders the task not easy under any circumstances; and when to this is added the repugnance that is felt by many females to manual and instrumental explorations, the difficulties are increased in no ordinary degree.

The methods of acquiring information regarding the several characters of the diseases of the uterus are, first, The history of the symptoms, and manual and digital examination; and secondly, The use of the speculum, the use of the stethoscope, and inspection and comparison of the discharges.

The history of the symptoms should embrace the whole account of the internal sensations and feelings of the patient, as well as her previous and antecedent feelings; the mode in which she became unwell, and observed the approach of feelings new, strange, and unusual; the presence and absence of pain, the character of pain, whether constant or intermittent, its imagined site, and all those circumstances which indicate more or less deviation from the state of health. Dr Ashwell remarks that in chronic structural disease of the uterus, there is seldom acute, early, or continued pain; while in functional disorders, such as Irritation and Inflammation, these conditions are invariably present. This is true. But often in chronic structural disease, there is dull, deep-seated pain of a gnawing character, endurable as to its actual sensation, but every now and then liable to become aggravated and intolerable, with pain in the back and loins, and an uneasy weary sensation, which obliges the individual to seek the horizontal position. It is further, not unusual to find that peculiar feelings of heat are complained of either in the pelvis or in the top of the thighs.

"*The pains dependent on increased bulk and displacement of the uterus*, are common to many, and diversified affections of the viscus. Supposing the uterus to have attained equal size, the painful indications will be nearly the same, whether the increased volume is produced by chronic vascular congestion, by hard fibrous or even calcareous tumours, by polypi, or by accumulations of fluid or air within its cavity. The suffering here is the consequence of mechanical pressure, and encroachments on neighbouring parts; and in all these cases, the patient will complain of dragging pain in the loins, extending occasionally to the anus and perinæum; of weight and fulness in the hypogastrium, with constipation of the bowels, if the uterus press on the rectum; or of difficult micturition, if it incline forwards and rest on the urethra or neck of the bladder. Again, should the uterus become largely distended at its sides, it may press on the obturator nerve; such pressure being indicated by screwing pains at the hips or inside of the thighs, or in any part of the course of the abductor muscles. A further increase of bulk may involve the sciatic; or, if above the pelvic brim, the anterior crural nerve;—points easily ascertained, by the pain being referred to the course of these nerves. But pains of a like kind are common in some of the displacements of the uterus, especially in prolapsus and procidentia. Nor are the sufferings occasionally attendant on the growth of the gravid womb very dissimilar. The distinction is not, however, difficult; the recumbent posture favours the return of a prolapsed, procident, or ante-verted uterus, and consequently suspends the pain; while the permanency of the morbid distensions and growths allows of scarcely any relief to the mechanical pressure, which, although slightly modified by an alteration of posture, is not removed. A variety of indications will ensure a correct opinion, in most cases of pregnancy. One, perhaps, deserves especial notice; viz., that the pain is rarely felt, in this state, during the gradual and natural distension of the womb, but during its premature or healthy contractions.

"The *duration* of the *pain*, and other *morbid changes* dependent upon it, deserve consideration. Whatever might be the symptoms, organic alteration would scarcely be suspected, except from the examples of some very rapid and rare malignant affections, where the suffering and other morbid symptoms had existed only for a few weeks. Such maladies, unlike inflammations and fevers, require months at least, and often years, for their full establishment. Eventually, and principally by mechanical pressure, the functions of other and neighbouring viscera are interrupted; disorganizing changes in their structure afterwards occurring, which ultimately affect the organic growths themselves. Nor, without the examination by touch, or the speculum, can we safely pronounce that there is no ulceration of the uterus, because there is no lancinating pain. It is true, that suffering of this kind generally accompanies ulceration; but numerous instances in the wards, and amongst the out-patients, attest that it is by no means an invariable concomitant. I have, every year, patients in the last stages of these diseases who still do not require opiates, so slight and transient is the pain caused by the ulceration.

"*Emaciation* is regarded as an almost unequivocal sign of structural lesion; and in the truly malignant diseases, it is seldom absent: but in hard tumours of the uterus, even of great size, there is often no emaciation, till the period when ulceration has occurred in neighbouring organs or tissues, and when the tumour itself is beginning to soften and break down. Where, however, the tumour is growing rapidly, or where it is producing irritation by mechanical pressure, digestion is soon impaired, there is little or no appetite, and flesh is sensibly and quickly lost."—(Pp. 255–257.)

The information furnished by digital examination is always important. There are two principal methods, the first externally above the pubes, the second by the vagina; to these may be added examination by the *rectum*, which is often serviceable in supplying additional and more positive information as to the state and position of the uterus, its magnitude, and whether its substance or interior be occupied by tumours.

The inquiry becomes increased in difficulty, when the age of the patient and other circumstances lead to the supposition, that she is pregnant. This question then assumes additional importance in a practical point of view. If there be no fœtus *in utero*, palliative treatment will be proper. If the patient be pregnant, her safety will mainly depend upon the induction of premature labour.

"The solving of this question requires that we ascertain the condition of the neck and mouth of the womb, the size and condition of its body, and the nature of the uterine contents, especially as to motion; if these various parts of the uterus are changed, as in simple and natural pregnancy, the opinion will be certain, and we shall proceed with confidence. The patient must be placed on her left side, the usual obstetric position; and the labia and nymphæ being carefully separated, the forefinger of the right hand will commonly reach and touch the parts satisfactorily. It must, however, be remembered, that the sensitive part of this finger can only examine with nicety that portion of the neck and os lying opposite to it; to examine the whole circumference of the neck, the index-finger of the left hand must also be used; and then it is scarcely possible that any morbid spot or induration can escape detection. As, in the operation of lithotomy, a deep perinæum increases the difficulty; so in the internal examination, an unusually long vagina, a broad perinæum, and large and fat labia, present obstacles to the investigation by a single finger. In such patients, two fingers, or perhaps the whole hand, must be used; having been previously lubricated by oil, rather than by any unctuous substance. If the neck be supple, broad, soft without tenderness, and if the *os* be closely sealed, so far the evidence is in favour of pregnancy. Doubts may arise here, because a polypus may distend the cavity, and lead to development of the neck, but the os would hardly be sealed; the neck itself may be the seat of chronic inflammation, or of hard tumour, generally diffused or confined to one spot, those may be puckered, fissured, or indurated by cicatrices, thus obscuring the indications; but, even here, tact will scarcely fail to appreciate in what degree the indications are to be relied on; the practitioner never forgetting, that all these morbid conditions may exist in the neck of a pregnant uterus. A sealed os would, in a case of such perplexity, avail much; tumours growing from the neck or os would perplex, but they would not greatly interfere with the opinion.

"The *second* part of this inquiry refers to the body of the uterus. And if the enlargement be globular, arising equally from every part of the circumference of the neck, expanding upwards after the manner of a balloon, affording an elastic resistance to pressure, such indications most probably result from pregnancy. Enlargement, it is true, may arise from hypertrophy, polypi, hydatids, and solid growths of various kinds; but here the resistance to the pressure of the finger would be different, there would be little or no elasticity, excepting where there are hydatids, for hydrometra or physometra need not be included; and there would be a solidity about the uterine mass, which could scarcely be mistaken for pregnancy.

"The *third* fact to be discovered is, the nature of the uterine contents, and especially whether they possess the power of motion. The stethoscope is not always available, even where the child is living, and where it is dead, it can afford no help. I have already alluded to the different kind of resistance to the pressure of the finger, on the lower part of the body of the uterus. Where there is fluid in the cavity of the womb, the fluctuation, and perhaps the sound, will afford tolerable evidence; and if the palm of the left hand be placed over the hypogastric region, when impulse is given to the fluid by the finger in the vagina, there will not be much doubt of its locality. If there be a fœtus in this fluid, the same impulse will cause it to rise, and having floated for an instant, it will again subside on the finger. This is termed "ballotement," or balancing the fœtus, and can be practised with equal facility, whether it be living or dead.

"*Examination by the rectum* will afford a correct idea of the degree of uterine enlargement, and of the pressure which it exerts posteriorly, and perhaps laterally. It may, too, confirm the impression of the solidity or fluidity of the uterine contents. The morbid peculiarities of the neck, already mentioned, may slightly interfere with the examination of the body; but not so seriously as to prevent our distinguishing whether the contents of the viscus are hard, heavy, and incompressible, or fluctuating and elastic.

"The placenta being entirely or even partially over the mouth of the womb, hypertrophy of the lower part of the organ, or one or two tumours, being situated in its front or sides, will, of course, prevent the certain conclusion which might otherwise be drawn; but we still have the history of the case, the previous pregnancies, if there have been any, the external abdominal enlargement, the examination by touch, and the stethoscope. This, then, is the inference fairly deducible—*that although there are cases where the able employment of all our diagnostic means fails to make us certain of the existence or non-existence of pregnancy, yet that such examples are very few, compared with the number in which such means, used with tact, will conduct us to a positive and correct opinion.*

"If the indications afforded by the vaginal examination, in doubtful and complicated pregnancy, approach thus near to certainty, *the touch, in most of the diseases of the cervix and os, aided by the speculum, must lead to a positive and correct opinion.* Often, however, the former mode will suffice, and it ought, in every instance, to be first employed. There is scarcely a case in which the examination by touch may not be used, once at least; while there are not a few in which the inspection by the speculum is absolutely injurious to the sound, as well as to the unhealthy structures of these organs. By the finger, we can correctly ascertain the size and shape, the consistency, the temperature, and the sensibility of the parts to be examined. Ulceration, or abrasion, may also be detected by the touch, but not with accuracy, as to its nature or extent."

The great use of the speculum is, to enable the physician to see the state of the *os uteri*, and the degree in which the neck and *os* are enlarged and detruded. It must not be imagined, however, that the use of this instrument conveys so much information as many of its partizans represent. Any one who is at all familiar with the normal condition of the *os uteri*, can determine at once without the speculum, whether the uterus is detruded, whether the *os* and *cervix* are indurated, or tuberculated, or whether there is a cauliflower excrescence, a fibrous tumour, or a polypus, quite as accurately by digital examination without the speculum as with that instrument. It is chiefly when the membrane forming the *os uteri* is abraded, ulcerated, or fissured, or is covered with aphthous vesicles, or is the seat of corroding ulcer, that the speculum communicates useful and positive information. On the state of the *cervix uteri*, that is the part above the *os*, it communicates little information; and on that of the body and *fundus uteri*, unless when the organ is inverted, it communicates none. Dr Ashwell, nevertheless, thinks that, by its means, different internal morbid conditions of the cervix may be accurately ascertained; an opinion which we think ascribes to its powers more merit than they possess. He adds, that growths of its lining membrane are not entirely beyond the reach of its diagnostic agency; because, if large, these growths descend towards the orifice of the organ; and if ulceration affect the uterine cavity, it is probable that it will eventually reach the neck, and thus be brought within the scope of the speculum. We must suppose that in this representation Dr Ashwell may be so far right. But it appears to be not a very legitimate, nor a very serviceable induction, to say that, providing the alleged lesion comes down beyond the orifice of the uterus, the speculum, which shews no more than this protruded portion, can communicate information as to the parts still contained within the uterus, and as to the state of the interior of the organ itself. It is manifest that it communicates no information; and all that the observer who employs it can do, is merely to form a conjecture.

It must be allowed, nevertheless, that the author is not an indiscriminate advocate for the use of the speculum; and he specifies various conditions and forms of disease in which its use is either not admissible or hurtful, and contraindicated.

"Valuable as is the speculum, its use has been indiscriminately and unnecessarily urged. In slight cases of uterine irritation and leucorrhœa, its employment is prejudicial; while; in leucorrhœal discharges of long standing, and in menorrhagia of months' and years' continuance, its introduction cannot be too strongly recommended: for it must never be forgotten, that these maladies rarely exist long, without more or less of organic change. If there be a suspicion of structural mischief about the lower part of the uterus, there ought to be no delay, not only in touching, but in seeing the seat of the suspected disease.

" There are circumstances which entirely forbid the employment of the speculum. In very young and very old persons, its introduction is difficult, and sometimes altogether impossible, without laceration. The hymen in the young, and the great shrinking and contraction of the vagina in aged women, present obstacles so serious, that the use of the speculum ought to be given up, unless the necessity be extremely urgent. I have several times found membranous bands stretching across the vagina, and contractions of its calibre from cicatrices, which would have entirely impeded the passage of the instrument. There was lately an out-patient of mine at Guy's, whose vagina was so funnel-shaped at its upper part, as to preclude my touching the os or cervix, except by a probe, introduced through the minute aperture at the apex of the funnel by which the catamenia escaped from the uterus. Steatomatous tumours occupying the walls of the vagina, ovarian growths in the recto-vaginal septum, polypi, deep ulcerations of the vagina or neck of the uterus, large cauliflower excrescences, or bleeding fungi, all contraindicate the use of the speculum. When the neck is inflamed, or much congested, or where the vagina is excessively sensitive, the introduction of the speculum should be deferred, till these various morbid conditions are ameliorated."

Attention to the condition and nature of the discharges is important. Yet these may be well observed, and the exact nature and state of the surface from which they proceed may be imperfectly understood. The great difficulty, which usually misleads not a few, is, that an opaque discharge, sometimes streaked, or even mingled, with blood, may proceed from a surface in a state of chronic inflammation, and may be supposed to denote the presence of disorganizing malignant disease; and, conversely, considerable organic disease may be present, yet induce not more than an offensive puriform discharge. Until examination has been allowed, says Dr Ashwell, a serous discharge has often been thought to be the proof of the presence of malignant disease of the *os uteri*, when it has only been leucorrhœal; and a purulent discharge, mixed with blood, and slightly odorous, has equally often excited painful anxiety lest organic disease existed, when in truth, neither by the finger, nor by the speculum, could such ominous disease be detected.

The existence of difficulties we allow. Yet we must say, that all of them either entirely disappear, or become greatly diminished to the practised eye and hand, and the mind trained in the school of clinical experience. The aspect of a female labouring under organic disease of the *uterus* is so characteristic, the emaciation so marked, the features so haggard and sickly, that, taken with the age, it is scarcely possible for a person of common observation to be mistaken and deceived. If he doubt for one moment, that doubt will be dispelled, by simple examination with the finger, with or without the speculum; and in some instances, even external examination alone is sufficient to make an experienced observer understand the nature of the disease. Hard adherent masses may be felt upon mere external palpation; and, by introducing the finger, he finds the whole orifice and neck of the *uterus* involved and converted into hard firm irregular morbid masses, which in one or more points have proceeded to ulceration. The form now described is associated in general with a period of life a little advanced, that is above forty; and age always ought to form an important element in establishing a diagnosis.

Let a different example be taken. A female, young, or at least not old, that is, between the age of nineteen and thirty, unmarried, or married, has enlargement of the abdomen. Of this, if unmarried, for obvious reasons, she says nothing for some time, so long at least as the swelling does not attract the attention of others. Then friends and observers begin to speak and whisper about the enlargement of ———, and remark that she looks as if she were in the family way, as they style it. The individual herself, abundantly distressed in mind, at length applies to the medical attendant, who, in some instances, may recognise the true nature of the case, in others, may not. In the latter case, he is unwilling to speak out, because he suspects, and sometimes pronounces pregnancy to be the cause of the swelling. At length various circumstances, and especially the declarations of the patient, lead her friends to entertain doubts as to the correctness of the opinion; and another practitioner is consulted. It is then found that disease of one of the ovaries has made considerable progress, and that the uterus is either not the seat of the swelling at all, or is thrust out of its place by the pressure of a greatly enlarged ovarian tumour.

In this case, one of two forms of disease may be present. *First*, either the ovary is converted into one or more sacs and cysts, some of these containing fluid; and the enlargement is the consequence of chronic inflammation attacking one or two of the Graafian vesicles, and causing the secretion of fluid which distends and enlarges them, while the stroma of the ovary is thickened; or, by some modification of morbid action, the cysts contain a quantity of thick jelly-like matter, which gives them externally the character of nearly solid tumours. *Secondly*, instead of this, a new heterologous deposit involves the whole ovary, and, increasing with rapidity, fills first the pelvic cavity and then the greater part of the abdomen. This is found to be Enkephaloma.

In neither of these cases, it may be argued, ought the mistake of supposing the cause of the swelling to be pregnancy to be committed. It is, nevertheless, a fact, that mistakes of the kind now mentioned are very frequently made, and the cases, as narrated, are given from actual occurrences. It is not of what ought to be that we now speak. It is of what actually is, and what most members of the profession, of any experience, can testify to be the case by personal testimony.

If it be said that the facts now adduced tend only to prove the necessity of the use of the speculum and similar means in diagnosis, we must be permitted to express a different opinion, and to say that they impress the necessity of giving the greatest attention to the rational as well as the physical signs, to the history of the case, the age and appearance of the patient; and that if from these a positive opinion cannot be formed, neither can it be formed from the use of the speculum.

The first subject which Dr Ashwell considers is that of TUMOURS OF THE WALLS OF THE UTERUS, OF WHICH THE LEADING CHARACTER IS INDURATION.

These he defines to be either of fibrous, cartilaginous, or calcareous hardness, varying in size from a pin or small nut to the volume of a pregnant uterus of the later months. They exert only slight influence on the constitution, and frequently their existence escapes notice, until by their magnitude they press upon neighbouring parts, mechanically interfering with their functions. Inflammation and its consequences may then ensue. Their malignancy is denied by many. But there is probably sufficient evidence to shew that they belong to the skirrhous variety of Karkinoma.

Of these growths, the author proposes to distinguish two forms or varieties. The *first* consists of tumours, which, whatever be their size or degree of hardness, grow externally, and, by causing the peritoneal coat of the uterus to project, obtain from it an external covering.

The *second* form comprehends those tumours which often, though not constantly, are of moderate induration and size, which, by pursuing in their growth an internal direction, carry before them, and are invested by, the uterine mucous membrane, and hence are usually known by the name of Submucous Tumours. These are accompanied by a class of symptoms entirely different.

These tumours have been long known, and received different names from different authors. By some they have been called the *fibrous or fleshy tubercle* when soft and compressible. If harder, they have been denominated *Skirrhous Cancer*, *Skirrhous uterine growth*, *Karkinoma simplex* vel *fibrosum*, *stone-cancer*, &c. The author thinks that they are to be regarded as malignant, though occupying the lowest place; that is, we understand, presenting the least marked characters among adventitious, heterologous formations.

The reasons which Dr Ashwell assigns for placing these fibrous and hard growths under the head of heterologous tissues, and looking on them as genuine cancerous productions, are the following.

1. They possess the structure of compound adventitious cysts, the basis of this class of heterologous formations.

2. In the colour of the contained mass, and in the arrangement of the membranous *septa*, or bands, the containing tissue, they are identical with skirrhus.

3. In degree of hardness, occasionally justifying the application to them of the term stone-cancer, they are not to be distinguished from the varieties of karkinoma already mentioned.

4. They occur very frequently in conjunction with growths of undoubted malignant character in other parts of the uterus.

5. And, *lastly*, they possess one especial attribute of malignant character, namely, incurability.

Dr Ashwell then proceeds to defend these views in the following manner.

> "It may be urged that, definite as are these conditions, the sum of them does not endow these growths with the power to assimilate different tissues to their own substance, nor with the capability to produce that peculiar cachexia by which cancer destroys life. No one can deny that the hard or fibrous tumour is the predominate organic disease of the walls of the uterus; and it must be granted, owing to their simpler structure as compared with the cervix, and the relation which always exists as to malignancy, between adventitious growths and the texture in which they are produced, that an indolent and benign character generally appertains to this variety. Still there are more points of resemblance to carcinoma than might be supposed; for these growths do occasionally disorganise the structure of the uterus, as may be seen by preparations in our different museums, and they often destroy life by that softening, degeneration, and break-

ing down, which can hardly be called ulceration, but which is peculiar to cancer. In many examples, also, one of which will be narrated, a fatal issue occurs in connection with, and apparently as the result of the cachexia of malignant disease. And, lastly, I believe it would be difficult, if not impossible, supposing the diseased masses to be removed from their respective sites, to distinguish between a so-called fibrous tumour and a genuine carcinomatous tubercle. On one occasion, I made sections of growths, coexisting in the same individual, taken from these distinct localities, and scarcely any appreciable difference could be discovered in their structure and arrangement; although, certainly, from the cut surface of the tumour of the cervix, there was a freer exudation of cancerous ichor. Notwithstanding these points of similarity, it is probable that the differences happily existing in the progress and termination of the two varieties will still be viewed by many as justifying the exclusion of fibrous tumours from the class of cancerous diseases."—(Pp. 290, 291.)

Of the correctness of this inference regarding the supposed malignant character of these tumours, we confess that we have considerable doubts. The whole question, indeed, resolves itself into the conditional definition of *fibrous tumour*, and what constitutes or does not constitute the fibrous tumour. But if it be the fibrous tumour, though its consistence may vary, it is not necessarily of skirrhous texture, and therefore not malignant. Here, also, we must observe, that the whole mode of speaking, by which these tumours are called either pleonastically *skirrhous cancer*, or even *skirrhus* or *cancer*, is, to speak most gently of it, neither scientific nor logical. The term *skirrhus*, though originally applied to all hard tumours, is now confined to designate solely that species of heterologous growth which invariably proceeds to cancerous ulceration. Neither is it correct to speak of *skirrhus* as being different from cancer, unless in the very trifling, and by no means essential circumstance, that *skirrhus* is the early stage of that which is vulgarly called cancer.

We feel the difficulty as well as the delicacy of controverting the opinions and distinctions of a writer, whose experience is so extensive as the present. But, as we sincerely believe, that these distinctions do not exactly express the present state of pathological opinion on this subject, and as the arguments employed, and the inferences given by Dr Ashwell, lead to important practical results, we feel it to be necessary here to make some remarks, which we hope, may tend to place the matter in its proper light. This is so much the more necessary, that it is evident that Dr Ashwell himself is in some respects conscious of the variance which subsists between certain of his pathological principles, and the application of these principles in actual practice.

We begin by remarking that we understand Skirrhus to be in all instances, a new growth, a heterologous structure, which is altogether incurable. That is, it consists of matter either different, or arranged in a different mode, from that forming any of the other regular tissues, and which possesses in itself no process by which the new growth may be removed, when once deposited. When the Skirrhous or Karkinomatous structure is deposited in any tissue, either superficially or through its substance, it cannot be removed, and is not removed either by the vessels of the system, or by any other process short of the complete and entire destruction of the part. This destruction, it is almost a truism to say, is the effect of its own onward tendency, the peculiar want of vital power in a structure totally different from and inferior to the normal structures. This destruction and disorganisation also takes place as a natural result of the low and imperfect structure by which skirrhus is characterised. This structure consists of the peculiar sort of cells called cancer cells and caudate bodies, the latter of which probably are formed in the course of the process of disintegration.

In the second place, though the peculiar structure of skirrhus is characterised by the presence of fibrous bands and septa, these are totally different from the fibrous structure of the pure fibrous tumour. The former presents the bands intersecting the structure. In the latter the whole is fibrous, but fibrous of a different kind entirely, not necessarily intersecting, but often constituting the great mass of the tumour in parallel or irregular layers and bundles. In skirrhus also, when there are white cartilaginous-looking bands, these present the cancer cells and caudate bodies. We are not aware that the latter have been recognised in the true fibrous or fibro-cartilaginous tumour.

In the third place, the fibrous tumour, and even the fleshy tumour, as it is not very correctly called, seem to possess, so far as hitherto known, no innate tendency, no necessary disposition to undergo the peculiar disintegrating process of skirrhus. They have been known to continue in a latent and inert state for years, and never to shew any tendency to the self-destroying process of cancer. When irritated, and when the circulation either of their own structure or the surrounding parts is disturbed, they cause inflammation and hæmorrhage, often profuse. But under proper management, they often return to their original state of quiescence, and remain so for years. By their position, their shape, and their size, they may compress bloodvessels, and cause hæmorrhage; they may compress nerves, and cause insensibility, or even some degree of palsy; but not necessarily do they destroy the textures with which they are in contact.

These reasons would, we think, be sufficient to make any reflecting person pause, before he referred these fibrous tumours to the same category as the skirrhous deposit or hard cancer. But another mode of reasoning has been often adopted, and we see that to this some countenance is given by the present author.

It has been said that in the female breast there have been observed hard tumours, which, by competent judges, have been pronounced to be skirrhous; and in which, so far has this opinion been carried, that it has been said that such breasts required to be removed by operation. From various causes, operation has not been resorted to; and the tumour has either disappeared, or has been so much reduced in size and become so free from pain, that after a considerable time, it has apparently been cured. At least no cancerous ulceration has followed, though life has been continued for years. Several cases of this kind we have seen and been concerned in. Not one of them has been genuine skirrhus. The majority were examples of strumous disease of the breast. These arise and increase under circumstances of impaired health, sometimes in connection with the catamenial secretion, generally in connection with ovarian and uterine irritation. They subside as health is improved. We have seen them subside under regulated diet, the judicious use of laxatives, the hot salt-water bath, and the internal use of iodine.

We regard it as altogether idle and unreasonable to compare and assimilate these tumours of the female breast to the growth of skirrhus. They take place in subjects totally different, generally at an earlier period of life, though this last circumstance is not universal. The skirrhous tumour admits of little alleviation. It sometimes becomes less painful. But as to hardness it continues the same. It causes inflammation of the surrounding tissues, adhesion in them, and finally detaches itself by ulceration and sloughing with great fetor. The scrofulous tumour even when it proceeds to suppuration, which it not unfrequently does, produces ulcers and sinuses, but not the dreadful excavations, sloughing, and fetid destruction of parts which take place in cancer. Neither have the sores the hard, elevated, everted border of the cancerous ulcer.

Lastly, there are not few nor doubtful facts to shew, that the skirrhous tumour depends upon previous contamination of the blood, and that the matter which causes the skirrhous tumour previously affects the whole system. When skirrhous matter is deposited in one organ or tissue, though it be removed from that by ablation, it re-appears either in that or in some other part. It gives also a peculiar character to the external appearance and general health.

With regard to the fibrous tumour of the uterus itself, we can assert from positive observation, that it does not produce either upon the external appearance or upon the general health those inauspicious impressions which are made in the case of cancer. When the fibrous tumour produces any impression upon the health, it is either by inducing hæmorrhage or inflammation, or by the pressure which it exerts upon bloodvessels, nerves, and other organs. But even under these circumstances, when under the use of remedies, especially rest, and the removal of sources of irritation, it has ceased to cause this irritation, and is reduced to its state of inertia, the patient recovers health and lives for years. The tumour, nevertheless, is not removed. It is perhaps diminished in size, and it no longer causes the same degree of local irritation which it previously did.

One of the most forcible arguments in favour of the opinion which we here advocate, and in opposition to that entertained by Dr Ashwell, is furnished by that gentleman himself, who in his therapeutic directions, proceeds upon the principle that tumours of the kind now mentioned are amenable to the resources of art. The author, it will subsequently be seen, recommends rest, local depletion, restricted diet, the use of local sedatives, and above all, the employment of iodine and hydrodate of potass. Now, it surely requires no reasoning to shew, that to none of these measures can skirrhus properly so called, be amenable. We take it for granted that the mere idea of producing any favourable impression upon a tumour, excludes the idea of skirrhus and all malignant disposition. Whatever therefore, may be the theoretical principles maintained by Dr Ashwell, it is clear that in practical application they entirely contradict the idea, that the class of tumours now referred to, namely the fibrous, and probably we may add the fleshy, though of this we are less certain, belong to the same category as those distinguished by the name of *Skirrhus*, *Karkinoma*, and Malignant Tumours.

It is quite possible that the arguments and considerations now mentioned, may have little weight against the testimony of the author. We desire not that they should have more weight than that to which they are justly entitled. It must not be denied also, that Dr Ashwell has on his side the evidence of a most competent witness, Dr Hodgkin, who in a sensible paper, introduced at page 377, expresses confidently his opinion, that all these fibrous growths are of the character called malignant. This letter we may probably at some future time give by itself. At present, while we refer to it as containing a good summary of the arguments on the side which maintain the malignant character; we must direct attention to the evidence of Mr Safford Lee.

Mr Safford Lee adopts the term Fibrous Tumour of the Uterus, to designate all these tumours, whether Fleshy Tubercle, Fibrous, or Skirrhous Tubercle of the Uterus.

The most usual situation for these tumours is in the submucous tissue, so as to project into the cavity of the womb; and their pedicles or stalks are generally situate immediately below the openings of the Fallopian tubes. The next position in which they are most abundant is the posterior wall and fundus of the uterus. They are rarely situate in the anterior wall, and still more rarely in the *cervix uteri*. These observations were obtained by Mr Safford Lee by the examination of seventy-four preparations in various collections of the metropolis. Of this number 19 were found distending the uterine cavity; 18 in the posterior wall; 18 in the fundus externally; 6 in the walls or substance of the fundus; 5 in all parts of the uterus; 4 in the anterior wall of the uterus; 4 in the posterior part of the cervix; and none in the anterior part.

When situate in the walls of the uterus, these tumours appear to occupy a space between its fibres, and are enclosed within a cyst, to which they are loosely connected, so that with the slightest force they are easily turned out of the cyst by the handle of the scalpel. These cysts vary in thickness; but they are always present.

The submucous tumours are liable to produce large and violent hæmorrhages, either at the catamenial period, or at intermediate times. They are hence not without danger.

The uterine tissue may or may not be increased in bulk around these tumours. It is opened or separated so as to receive the morbid growth. But when the tumours project into the cavity, and either assume the polypoid appearance or are attached by a broad base, they distend the cavity of the uterus, which acquires the same action as in pregnancy, its walls becoming thicker, as during that state. Mr Safford Lee allows, nevertheless, that the increase in the uterine tissue is not constant; and he states that in the Museum of Guy's Hospital, in several of the preparations, the increase in uterine substance is very great, while in others the fibrous structure over the tumour is attenuated.

The tumour itself is hard and smooth on the surface; in its natural figure globular or spheroidal, but liable to vary. The interior substance is of a pale ash colour, intersected by white shining interlacing lines, forming interspaces which are filled by dirty white matter. On the nature of this, different opinions are entertained. Lisfranc considers it to be plastic lymph; Leveret compares it to the udder of the cow; M. Roux thinks it resembles the intervertebral cartilages of the aged. Dr Rigby thinks that the hard white lines which intersect these tumours consist of nearly pure cartilage: the grey-coloured soft portions between them, which form the bulk of the tumour, seem also to consist of cartilaginous structure, intermingled with cellular tissue. The fleshy appearance though met with is not common.

"I have examined," says Mr Safford Lee, "many portions of these tumours from various situations of the uterus, by the microscope, and I find that they invariably present a cellulo-fibrous appearance. From a part of the central tumour (No. 1), which accompanied the Essay, three different degrees of the same object were observed: in one portion the cellular tissue predominated; in another the fibrous tissue, combined with cells; and in a third, the true-looped fibrous tissue, radiating from a centre, and diverging into a form resembling the star-fish. No other cell, except the simple nucleated cell, could be distinguished. Dr Oldham states, that the anatomical elements of the fibrous growth are a clear unstriped fibre, closely packed, and interspersed in some instances with crystalline calcareous grains (the existence of which has long been known as a chemical constituent of them), and minutely divided arteries. These growths may degenerate into cartilaginous or bony deposits.

"When these tumours have remained for many years in a dormant state, they are frequently found to have undergone changes in their structure; their minute tissue becomes much denser, cartilage becomes deposited in isolated points, which grates under the knife, and assumes a horny and semitransparent state when dried; and here and there actual spicula of bone are observed. I believe this is the ordinary mode by which bone is deposited; although some think that all previous changes can be dispensed with, and that bone can be deposited at once. In the Museum of the Royal College of Surgeons, England, is a beautiful preparation (*Uterus* 15), which well illustrates the former views: a tumour of the uterus is laid open, and presents points of cartilage and bone interspersed in all directions; and in many parts bone is seen deposited in the centre of a small portion of cartilage, which more properly is undergoing the bony degeneration. Lisfranc also states, 'I have shewn a womb of enormous size, extracted from the pelvis of an old woman; upon whom I found fibrous tumors *not* degenerated, bony tumours, and cartilaginous tumours.'

"The bony transformation is very hard, hardly to be cut with a knife, and occasionally it is so compact as to be able to receive a fine polish: this is seen in Guy's Museum. This deposit may commence (as we have seen) in all parts of the tumour; may do so in the centre, and sometimes it only attacks the circumference."—(Pp. 6, 7.)

When tumours of this kind project into the cavity of the uterus, and undergo the bony transformation, they may become detached and be expelled from it; and of this expulsion many instances are recorded. The bony change produces no derangement, either in the general health, or in the function of the organ.

These tumours consist of the phosphate and triple phosphate of lime, with a little carbonate and phosphate of soda mixed with cellular tissue and animal matter, in variable proportions.

Fibrous tumours of the uterus are of themselves insensible.

Some are capable of being injected. But the vessels which they receive are small in size and few in number; and others of these tumours it is impossible to inject. The vessels which penetrate the tumour proceed from the surrounding cellular cyst, which is very vascular; and it is from this texture that arise those profuse and repeated hæmorrhages which take place when the tumour protrudes into the cavity of the womb. This circumstance explains the reason why these bodies do not undergo the changes consequent on inflammation, but only those of disorganization. If inflammatory action arises in the cyst and the surrounding tissues of the womb, the small vessels passing to the tumour are obliterated, and the tumour, deprived of blood, dies.

No veins are observed in the interior of these tumours; but upon the surface they are large and varicose. When injected, the matter does not pass into the substance of the tumour.

"Many pathologists of the highest reputation state that the hard tumour of the uterus is entirely unconnected with cancer; and some go as far as to class them with those tumours which are unable to become cancerous; while there are others equally eminent who say that these tumours are cancerous, but of the lowest type. I do not myself think that they are. The facts obtained by the daily observation of these tumours tend to prove them quite benign and harmless. They frequently exist without observation during life, nay, they may even pass to their ultimate development, viz. ossification, without giving any signs indicative of their presence. They rarely, if ever, ulcerate, and are quite distinct from the neighbouring tissue. They kill by the obstruction of contiguous organs, not by the extension of the disease.

"The termination of these tumours is various. They may either become, as it were, dormant, and produce no irritation whatever in the parts; they may change their structure, and become converted into a bony mass, or they may produce such irritation in the uterine tissue, as to produce inflammation and abscess in it, and thus become discharged. On the first two points we have said sufficient; the latter requires remark. This is not of a very frequent occurrence, and produces almost always fatal effects, although occasionally the patients recover. It generally occurs after labour, when the tumour has been pressed upon, and the uterus prone to inflammation. I had an opportunity of examining one of these tumours thus discharged, anatomically and microscopically. It presented all the appearance of a fibrous tumour after it had been macerated in water for some time. This patient recovered."—(Pp. 9, 10.)

To the foregoing statement it is unnecessary to make any addition.

It is by no means frequent for the presence of these tumours to interfere with impregnation. They do so occasionally, and consequently they must be regarded as one cause of barrenness. But there are many instances recorded in which the patient became pregnant, and in which the tumour formed a serious and troublesome complication in parturition. The most frequent hazard is the subsequent occurrence of profuse and violent hæmorrhage, which has been known to terminate life. This is an immediate result. More remote effects are breaking up of the tumour, inflammation and abscess of the womb.

The following account is given of the particular symptoms and effects produced by tumours of this kind.

"1. Those tumours which are placed under the peritoneal surface can be distinctly felt by the hand on external abdominal examination. They may be either smooth or lobulated, and generally, though not always, have a fixed character; but when they possess a pedicle, and it is long, they can be moved about the cavity of the abdomen like a diseased ovarium. If their growth is rapid, they are accompanied with indurations of the uterine tissue around their seat; you then have great pain on pressure, with symptoms of general disturbance, more or less fever, with quick pulse, thirst, and derangement of the digestive system. These symptoms are so frequent when the tumour occupies this position, as almost to be pathognomic of the disease. 2dly, If the tumours are placed in the posterior or lateral parts of the uterus, the early symptoms are more distressing; you have then great pain from pressure on the pelvic nerves, the veins in the abdomen become pressed upon, œdema and varicose veins shew themselves, more particularly on one side, sometimes on both; piles are produced; the evacuations of the rectum are interfered with, the fæces are flattened, and constipation, frequently the most obstinate and troublesome symptom, is present, and produces inconveniences we can hardly obviate. 3dly, If the tumours are placed in the anterior walls of the uterus, the functions of the bladder become more particularly implicated. The urine can be retained only for a short time; there is almost constant desire to make water, although only a small quantity is evacuated. At other times retention of urine occurs, which is relieved by change of posture or the catheter. Piles are a very frequent accompaniment of tumours of the uterus. 4thly, If attached to the fundus, it may cause retroversion of the womb (Clarke), and prevent the evacuation of fæces and urine at the same time. Dr Lever relates a case of this sort, 'where the obstruction had been so great, that the bladder, ureters, and pelvis of the kidneys were enormously distended, whilst the bowels were loaded with masses of hard fæculant matter.'

"2. When the tumours are placed in the walls of the uterus, these symptoms are much more obscure, until they obtain consideration from their bulk. In many instances, the only symptoms at all referable to the uterus are, a weight in the seat of that organ, with occasionally a burning sensation, accompanied by an increased mucous discharge. This may remain for years without producing any further inconvenience, until some cause or other produces irritation; it then increases and produces all the symptoms of pressure on the neighbouring organs, which have been already referred to. When in this position, they may even become osseous without attracting attention.

"3. Tumours projecting into the cavity of the uterus are much more serious in their nature, and produce more immediate effects, than those just described. Of these there are two varieties,—1st, Those which retain their character as tumours, but project into the cavity of the uterus. 2dly, Those that loose that character, project so far as to obtain a peduncular attachment to the womb, and in fact become polypoid tumours. These latter, with their symptoms, will be noticed when speaking of polypi.

"When the tumour is submucous, and only projects into the cavity, and not so far as to become a polypoid tumour, the first symptoms usually observed are those connected with derangement of the function of menstruation. The patient finds that the periodical discharge is increased in its quantity, lasts longer than usual, and is followed by a more or less white discharge. This may or may not call attention to the uterus; but soon the symptoms are aggravated, the quantity of discharge is not only increased, but contains clots, a considerable portion of blood is lost at each period, and these occur more frequently, until at last the constitution begins to suffer.

"The menstruation is variously affected in regard to tumours of the uterus. Thus, when the morbid growth is situated in the walls of the uterus, that function may be natural at first, and remain so up to a late period in the disease; but when it projects into the cavity of the uterus, it invariably gives rise to bleedings to a greater or less extent, which usually occur at the menstrual periods.

"When the tumour projects into the cavity of the womb, the patient is greatly harassed by expulsive pains. These may be almost constant, but generally intermittent; they simulate labour pains, and, when absent, leave a dragging sensation in the back and loins, running down the thighs. They most usually occur when the menses are about to make their appearance, and continue until they have ceased. They are so severe as to incapacitate the patient for her usual duties."—(Pp. 12–14.)

Dr Ashwell, it has been already seen, forms a second division of these tumours, namely, the Submucous; and to these he ascribes a character still more serious, and consequences still more dangerous.

These are chiefly remarkable for the profuse and repeated bleeding which their presence induces. From the account given by Dr Ashwell, it is evident that they correspond with that order of tumours already noticed as mentioned by Mr Safford Lee. Dr Ashwell's opinion is, that the bleeding is principally, if not entirely, furnished by the tissue covering the surface of the tumour, rather than by the tumour itself.

> "The peculiarity of these examples consists in the occurrence of the bleeding prior to ulceration; so that we must always bear in mind, in hæmorrhage from the unimpregnated uterus of unusual frequency, resisting the most judicious and persevering treatment, that there may be a tumour of the kind now described, distending the cavity, and out of the reach of the finger, maintaining so congested and inflammatory a condition of the mucous membrane, as almost to render these bleedings necessary for its partial relief.
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> "The statistical facts already adduced lead to the belief, that these are, happily, singular cases; yet it is valuable to know that such a cause really exists, as it may lead to greater diligence in the use of early, and perhaps antiphlogistic remedial measures."—(Pp. 294, 295.)

Dr Ashwell calls in question the correctness of the opinion which regards these tumours as similar to what are called Polypi.

> "That there are points of resemblance between the two diseases, although these have probably been overrated, I do not question; but that a genuine hard or fibrous tumour ever becomes a pediculated polypus, I can scarcely believe. A specimen of such a change Guy's Hospital Museum does not supply. That one of these hard fibrous tumours may very rarely find its way into the uterine cavity is allowed, because the statistics of the disease prove it; and that prior to the patient's life or her powers being destroyed by the bleedings, which, in this situation, the tumour may occasion, the growth may, as a *most unusual* occurrence, descend to the lowest part of the uterine cavity, distend and pass through its cervix, and ultimately find its way into the vagina, may also be conceded; but it will be a hard or fibrous tumour still, although its altered situation, and the bleedings attendant upon it, may justify and even demand its removal by the same means as in polypus.
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> "In *structure*, with some few exceptions; in *sensibility*, both in the growth and the surrounding parts; and in *vascularity*, as well as in *many other particulars;* there is a marked difference between the hard or fibrous tumours of the cavity now described, and polypus.
>
> "An inspection of preparations of such morbid growths cannot fail to demonstrate how much more distinct, generally, in the tumour, than in the polypus, is the induration of texture; and certainly the white membranous lines are much more defined and striking in the former than in the polypus. Thus, while it is somewhat rare, except

in old, large, and condensed polypi, to find this indurated and linear structure, a genuine, hard, or fibrous tumour, except when breaking down, is never without it.

"In the number of the growths there is decided dissimilarity. It is common to meet with many hard tumours in the same uterus, while it is very rare to find more than one polypus.

"In the method of growth there is a conspicuous distinction. The polypus, probably because it is not malignant, does not affect the organisation of surrounding parts, the muscular walls of the uterus being rarely thickened, however large may be the polypus. The hard tumour, on the contrary, may, and often does, convert by degrees the uterus in its vicinity into its own diseased structure.

"The internal tissue of many polypi is spongy and cellular, and copiously permeable by blood, a circumstance scarcely ever appertaining to the hard or fibrous tumour. I was much struck, a few days ago, by a preparation in the Museum of St Bartholomew's Hospital, where a hard tumour, imbedded in the walls, had received no injection, although the vessels in every other part of the uterus were beautifully filled, a circumstance lending something more than probability to the opinion I have just now expressed, that the hæmorrhage, in these instances, is furnished by the membrane covering the tumour, and not by the growth itself; while in the polypus probably with very few exceptions the bleeding occurs from the vessels in its structure, as is satisfactorily proved by its texture, and by the difficulty of getting any mercury or other injection retained in its vessels, however carefully it may be thrown in. In the polypus injected by Mr Sibson and myself the mercury quickly escaped through the orifices of the vessels opening on its surface. Sir Charles Clarke affirms, that if coloured injection be thrown into the vessels of the uterus, so as to make the substance of the uterus quite red, none of it passes to the tumour of fleshy or hard tubercle.

"The want of sensibility is an almost invariable condition of true polypus, while the hard or fibrous tumour is never entirely bereft of sensation.

"Pregnancy may, and often does occur in connection with hard or fibrous tumours; rarely, if ever, when there is polypus, except where the growth arises from the cervix or os. There are, so far as I know, very few such pregnancies on record."—(Pp. 295–297.)

The treatment of these tumours Dr Ashwell allows not to be peculiarly satisfactory. Palliation, he thinks, is all that can be promised. He records, nevertheless, several instances in which the patients recovered from the immediate hurtful effects. The remedial measures recommended and most likely to prove beneficial are rest, self-denial of every kind, restricted diet, the occasional application of leeches, the local use of iodine externally, the internal use of iodine and hydriodate of potass, and due regulation of the bowels either by means of enemata, or aperient medicine of the least stimulating property.

Under the use of these measures, either the tumour ceases to enlarge, or it is diminished; the health and strength of the patient are re-established; and life may be prolonged for years after the existence of the tumour has been fully recognised. The tumour itself does not disappear unless in rare and exceptional cases. But it gets into an inert state. Dr Ashwell gives one instance (Case 47) in which a hard tumour was cured by spontaneous disruption of its structure and discharge of the fragments.

It has been already stated that the presence of these growths does not in all cases interfere with impregnation; and the complication of pregnancy with the presence of these tumours constitutes one of the most serious difficulties with which the obstetric practitioner has to contend.

Patients becoming pregnant in such circumstances are placed in great danger. The tumour or tumours soften during the latter months; the increased supply of blood leads to inflammation; unhealthy suppuration is established; and death takes place soon after parturition.

In order to obviate and prevent the occurrence of casualties of this description, Dr Ashwell proposes the induction of premature labour before that period, when the tumours shall be subjected to pressure and contusion, from the firm, large, and unyielding gravid uterus.

The reasons which Dr Ashwell assigns for this practice are quite satisfactory. *First*, when death occurs after a labour so complicated, the result is only in a slight degree, if at all, to be referred to the state of the uterus, which rarely sustains any serious mischief. The fatal event, on the other hand, is produced by inflammation, softening, and unhealthy suppuration in the growth itself; pathological changes which, in some instances, lead to rapid sinking; while in other instances in which the powers of the system have been less impaired, life is terminated in a few days, from the constitutional shock induced by the protracted duration and difficulty of parturition, and by the contusion and injury done to the tumour and other soft parts. In the *second* place, premature parturition artificially induced rarely occasions constitutional disorder; it is easily accomplished; and it affords the best, and in many instances the only chance of a safe result to the mother.

"Professional attention has not hitherto been sufficiently directed to the agency of the tumour itself, in the production of the fatal event. Of course, it has not been overlooked, that it is the cause of the difficulty in parturition; but it seems to have been almost forgotten how well women gradually recover from protracted, difficult, and instrumental labours, where there is no tumour, or adventitious growth of large size. A due estimation of this fact would, I believe, ere this, have led to the practice I propose. A careful reader of the narratives of labours thus complicated, and of the rapid dissolution following them, will discern, to a certain extent, a parallel as to the fatal sinking, between them and labours where the use of instruments has been too long deferred. I have several times witnessed the loss of life in a few hours after delivery by the perforator, where sixty or seventy hours had been improperly allowed to elapse before its employment; the impression on the system being very similar in both instances.

"In all the fatal instances of pregnancy complicated with tumour which I have seen, and where I have had the opportunity of examining the parts after death, the uterus itself has been healthy, or very nearly so; and, in the majority of those recorded, this viscus is reported to have been either in a natural state, or free from disease."—(Pp. 324, 325.)

These inferences are further corroborated by the testimony of Dr Merriman, Mr Hewlett, and other practitioners.

As to the propriety of inducing premature labour in these cases, and its safety, Dr Ashwell adduces evidence of the most conclusive character. The objections which are urged against the operation he regards as too trifling, and applying to symptoms too fleeting and transitory to be allowed to act as serious obstacles. These objections apply, he thinks, mostly to symptoms of nervous irritation. Further, the operation has been recommended and practised by several judicious, and conscientious practitioners, in those cases in which the pelvis is unusually small, and has a narrow or deformed outlet. This argument, therefore, the author urges with force, and, it must be allowed, justly. As the dangers, he argues, of labour in cases of narrow and deformed pelvis are thus happily avoided by the practice of premature parturition, artificially produced, why should not the same method be adopted in order to obviate the analogous dangers of pregnancy complicated with the presence of tumours. The life of the fœtus, he admits, is endangered; but this, he maintains, should not be placed in competition with the safety of the mother.

Dr Ashwell, nevertheless, is not an advocate for the indiscriminate adoption of this method in instances of pregnancy complicated with tumour and organic disease. *First*, the full time of pregnancy may have been completed before the attention of the practitioner is directed to the existence of a large abdominal or pelvic growth. In this case the procedure of artificially induced parturition is not admissible. The accoucheur must trust to circumstances, and must exercise both judgment, patience, and consideration, If the tumour be moveable; if it be so situate that the enlarging and hard uterus has pushed it out of the way, and makes upon it no painful pressure; if it be the first labour so complicated; or if it be the second or third, the growth not having increased during the intervals of the successive pregnancies; then it is best not to interfere. If, however, the opposite of the circumstances now mentioned take place, it would be unwise to incur the risk which a labour, to be naturally prosecuted at the full time, would not fail to induce. *Secondly*, when the growth is small, and only of moderate dimensions, or so placed that it may not probably interfere with labour, the attendant, Dr Ashwell thinks, may leave the process to nature.

Here again a considerable difficulty occurs. An obstetrical attendant is called to a female on the eve of labour, and recognises then, for the first time, some unusual difficulty. Probably he finds one tumour or more in the uterus, or connected with it, and impeding or likely to impede labour. He may request the advice and assistance of another practitioner in whose knowledge and experience he has confidence. But though this gentleman may be conscientiously of opinion, that had either he or the other gentleman known the peculiarities of the case one month or six weeks earlier, it might then have been practicable to have induced premature labour with good results, it is no longer possible either to accelerate labour with advantage, or to interfere in any way. The patient must then be left to her fate, or delivery by instruments must be attempted.

There is also a class of cases in which even the method proposed by Dr Ashwell, though practicable, will not accomplish the purpose intended. The ovaries may be in a state of disease so considerable as to render all attempts to induce parturition ambiguous and dangerous. Such appears to have been the state of the parts in the case recorded by Mr Hewlett, in the seventeenth volume of the *Medico-Chirurgical Transactions*, in which both ovaries were affected with what is there pronounced to be malignant disease.

Dr Ashwell is at great pains to place this piece of practice in the clearest light, and to shew that, while delay in this case is dangerous, the induction of labour six or seven weeks before the full time, in cases where the existence of considerable organic disease is proved, promises the most likely method of carrying the mother safely through the process. The final inferences are stated in the following manner.

"I propose, then, to evade the dangers of inflammation of the pelvic tissues and peritoneum, and of the still more hazardous evils of unhealthy softening, suppuration, and ulceration of the tumours themselves, by the simple and safe expedient of premature labour; a practice accidentally suggested to me, and enforced, on reflection, by the fact that death had frequently occurred where pregnancy was so complicated, under the best known treatment, exclusively of premature labour artificially or spontaneously induced. Nine out of the eighteen cases which fell under the notice of Dr Merriman terminated fatally: Mr Hewlett's case was equally unsuccessful; and three of my own patients fell victims to grangrenous inflammation of the tumours, produced by their contusion during the process of delivery. Worse results could not have followed premature labour; and I feel confident that, had it been induced, several lives might have been saved. It may be inquired whether there was not sufficient mischief done to the uterus, in these cases, to insure a fatal event? Certainly not. In most of the cases mentioned by Dr Merriman, there is no allusion to the condition of this viscus; and when there is, it is stated, with one exception only, to have been healthy. In Mr Hewlett's and my own examples, the womb was free from inflammation. I believe these patients to have been destroyed, as others will be, where this practice is not adopted, by morbid and malignant changes in the tumours themselves; collapse and final sinking having been induced, much in the same way as after pressure and strangulation of an intestine, or after contusion of the soft parts in difficult labour, where an inspection after death commonly brings into view intense inflammation, and sometimes gangrene and disintegration.

"A corroboration of the propriety of the measure is furnished by the marked success attendant on puncturing the tumours, especially where their contents were fluid, or viscid; and, in one or two instances where blood only escaped. By this operation, the bulk and tension of the tumour were diminished, and the double purpose was accomplished, of a partial removal of the obstacle delaying parturition, as well as a preservation of the growth itself from that severe pressure and contusion which may lead to rupture of its parietes, and inflammation or gangrene of its substance.

"Six cases published by Mr Park of Liverpool, in the second volume of the Medico-Chirurgical Transactions, are equally in point. Puncturing the tumours was the most successful of all the measures adopted; and it is worthy of observation, that the only fatal termination occurred in a case where the tumour, occupying the recto-vaginal septum, was subjected to the pressure and contusion of the fœtal cranium for three days. Delivery was eventually accomplished by the natural efforts, and the patient died in twenty-four hours, from vomiting and constipation.

"A perusal of all that has hitherto been written on the subject will satisfy any one, that the procedure to be adopted in these unfortunate complications, at the time of labour, is by no means clearly defined. If the opposing growth can be pushed above the brim, the difficulty is at an end; but if it cannot be so raised, although puncture of the morbid structure is the best remedy for tumours with fluid contents, it will avail little in the management of solid and very hard growths. Extirpation by the knife may be thought of; but the connections of the tumour, the shock of the operation, the probable hæmorrhage, and the subsequent inflammation, are events too certain and too hazardous to allow of a favourable prognosis.

"These remarks are strictly applicable to morbid enlargements opposing the descent of the child; but they are equally pertinent to hard and malignant tumours of the uterus itself, and to ovarian and other growths of such magnitude and firmness of attachment, as to preclude the possibility (*vide* Mr Hewlett's and my own cases) of their being lodged in the abdominal and pelvic cavities, together with the gravid womb, without exciting pressure and contusion. This observation is especially true where pregnancy is complicated with one or more hard tumours imbedded in the parietes. If the practice proposed were dangerous to the mother; if it increased her risk at the time, or if afterwards it placed her in a worse position than she had previously occupied—objections to it would carry great weight. It is, however, satisfactory to know that none of these evils are the effect of premature labour artificially induced.

"I think, therefore, I may regard as proved, that great advantage will accrue from this method, where the tumour cannot be raised above the brim; where it is situated in the abdomen, and is of such size as to restrain the development of the uterus without painful pressure and contusion, or where the growths occupy the uterus itself.

"Before concluding this chapter, I must allude to other affections complicating pregnancy, which, from the aggravated evils they produce, fully justify this practice. I do this with more satisfaction, as I find that Dr Robert Lee, in a paper recently published in the *Medical Gazette*, confirms the value of the measure, so long recommended by myself, by a series of cases, of all which he remarks, 'that premature labour was, or might have been employed with advantage.'

"In the list of such maladies must be included cancer of the *os* and *cervix uteri*, especially in the early stage, and even when destructive ulceration has occurred; corroding ulcer and cauliflower excrescence of the os; diseases of the vagina, urethra, labia, and perineum; in all which, with varying probability of benefit, and in some with scarcely more than transient good, the measure may be adopted. From what has been said, it may be inferred, that circumstances must modify the propriety of the practice, and that the ultimate decision must depend on the judgment of the practitioner.

"There can be little doubt, that this practice will hereafter be generally adopted; where ovarian dropsy or ascites, organic affections of the heart and excessive sickness, render the continuance of pregnancy a matter of serious risk."—(Pp. 333–336.)

On the subject of Cancer of the Uterus, Dr Ashwell makes many interesting observations. But the interest mainly consists in the view, to which he is not a little inclined, that the disease admits, in the early stage and in certain circumstances, of cure.

Two points he allows to be established; *first*, that the disease is malignant; and, *secondly*, that its especial seat is the neck of the uterus.

A third question he considers to admit of doubt, to be in truth an open question. This is whether, presuming the disease to be in its incipient stage, it is reasonable to hope for prevention of further mischief, or a cure for that which already exists. It is manifest that to state such a question, implies that in the incipient stage the disease admits of cure. Some notion of this kind was entertained and expressed with sufficient distinctness by Madame Boivin and M. Duges, who had the idea, that in the early stage, it was possible to prevent the further progress of the disease, and possibly thereby to effect a cure by means calculated to abate chronic inflammation; for instance, the repeated application of leeches, the employment of enemata and aperients, rest, soothing sedative lotions, and regulated unstimulating diet; in short, all means of withdrawing stimuli and preventing their operation.

This doctrine these authors announced in the second volume of their Practical Treatise on the Diseases of the Womb and its Appendages, published at Paris in 1833; and of the doctrine, the proofs on which it rests, and its applications to practice, an account sufficiently ample was given in the fortieth volume of this Journal.* We have always professed to be the historians rather than the judges, both of speculative views and practical measures; nor did we at the time referred to, express any very strong opinion. It was indeed, neither the season nor the place to do so. At present we may remark, that since that time nineteen years have elapsed; and evidence either in favour of the correctness of these doctrines or against them, ought to be sufficient, in force and in amount, to shew whether they are so far well founded, as to entitle them to the confidence claimed for them. As it is not a matter of doubt, that the question can be decided only by extended experience and its results, the experience of Dr Ashwell is entitled to serious attention.

Of the correctness of these opinions, we confess we always entertained doubts; because it appeared on the other hand not clear, that the alleged cases of skirrhus or cancer were anything except examples of neglected chronic inflammation of the *cervix* and *os uteri;* and, because, on the other hand, we have a most firmly founded persuasion that the skirrhous structure, in no circumstances admits of being cured or removed by the powers of the system, of undergoing absorption, or of being eliminated without completely infecting and destroying the contiguous tissue.

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* Traité Pratique des Maladies de l'Uterus et de ses Annexes. Par Mad. Boivin, M.D., et A. Duges. Vol. ii. Paris, 1833; and Edinburgh Medical and Surgical Journal, vol. xl., October 1833, p. 366, &c.

Dr Ashwell, we perceive, is guided by the same or similar notions, as those entertained by Madame Boivin and M. Duges, and since, we believe, more or less decidedly advocated by Duparcque and Montgomery. He allows that it has been doubted, whether he has sufficiently defined the nature of these hard tumours, whether they are to be regarded as cancerous, or merely as congestions and ulcerations, which not being malignant, are capable of cure. "I believed at the time I made these observations, those, namely, in Guy's Hospital Reports, and I still adhere to the opinion, that they were malignant tumours; but that their full development was prevented at this early period by the treatment pursued; for I have long been convinced that cancer of the womb may be arrested in its early stages by the removal of the pathological state, of which it is the consequence."

This is equivalent to saying that cancer of the womb is the effect and consequence of a morbid state, which is amenable to remedies, and may be removed by appropriate treatment. Now, we must be permitted to say, that all this confusion proceeds from not defining correctly the original terms employed, and from confounding, as the same, two morbid states which we think may be proved to be different. Dr Ashwell, it has been seen, considers the hard Submucous Tumours of the womb as of the same kind as the malignant, in other words, as the same as the Skirrhous tumour, and proceeding to cancerous ulceration. Conversely, the Skirrhous tumour, though he allows it to be malignant, he thinks to be at an incipient stage capable of cure. If we understand him aright, he takes from *Skirrhus* and *Karkinoma*, a certain part of their attribute, and this he gives to the hard or Fibrous Tumour of the uterus, to which it is not certain that it belongs.

The author proceeds in the following manner.

"To suppose, or to call these hard tumours skirrhous, cancerous, or malignant, would in some minds instantly excite prejudice. If I am censured, then, for using the term 'hard,' I justify myself by saying, that it is the best and least controvertible expression with which I am acquainted. It is scarcely possible to avoid attaching a precise, and perhaps an erroneous idea, to such terms as 'skirrhous,' 'cancerous,' or 'tubercular indurations.' The denomination 'hard tumour,' has this advantage;—it assumes only a degree of hardness, or firmness, beyond that which is healthy and natural, leaving the precise cause of such hardness to be decided by the result of the treatment, or to the further progress of the disease. Such a condition may be the effect of chronic inflammation only; or, if of malignant character, it may yet be very distant from that degree of malignancy which will resist all treatment."—(P. 370.)

It must be allowed, nevertheless, that Dr Ashwell expresses himself diffidently, and looks rather to the practical bearings than the theoretical justice of the question.

" Nevertheless, I am persuaded, if many of these structural changes (in the os and cervix) were examined without reference to their treatment at all, and especially by iodine, they would be pronounced to be scirrhous or malignant alterations. I am not, however, pertinacious on this point: it is not a matter of practical moment; although my conviction decidedly is, that these changes, whatever may have been their precise character at the commencement of the iodine treatment, would, without that treatment, have proceeded on to ulceration, and thus have left the patients with but a slight chance of recovery.

" Until I employed the iodine, especially in the forms of ointment and tincture, directly to the diseased growths, and the treatment already pointed out, I saw these indurations gradually getting worse: I perceived them slowly softening; till at length their surfaces were broken, and ulceration occurred. It is needless to say, that, after this event, constant irritation, fetid and sanious discharges, and occasional hæmorrhages, sooner or later induced a fatal result. I have examined many of these structural alterations with great care, both by the finger and the speculum; and, after repeated investigations, extending over several years, I am not disposed to think less favourably of the treatment.

" In these views Duparcque and Montgomery fully coincide, and I regard it as of great moment, that in a matter so truly important as the prophylactic treatment of cancer of the womb, the experience of these distinguished physicians should coincide with my own. The former has arrived at the following conclusions.

" 1. The greater part of confirmed cancers of the womb succeed to congestions and ulcerations capable of being cured; we may then, to a certain degree, prevent the development of these maladies by properly treating, at an early period, the primary pathological states of which they are the consequence.

" 2. Once fully developed, confirmed cancers are, at present, beyond the resources of medicine; even surgical treatment, which offers some chance when the disease is limited to the neck of the uterus, is of no service when the entire organ is affected.

" 3. In all cases, a well directed palliative treatment of symptoms will arrest the progress of the complaint; render it in some degree stationary, and relieve the most painful symptoms and the gravest 'accidents,' or at least so far mitigate them as to render less painful the approach of death.

" 4. All the cases of extirpation which have been published, were so at a period too near the time of the operation (four, five, or six months at most), for us to judge fairly of it. It is probable that a greater delay would have afforded even less encouragement."—(P. 371, 372.)

To the same purpose the author quotes the testimony of Dr Montgomery, which is in substance, that there is an early stage of cancer preceding the two described by authors, in which the nature of the disease may be detected, and proper remedies may be applied to arrest its progress and prevent it from advancing to the stage of disorganisation. This early stage consists in chronic inflammation with hardening and enlargement taking place chiefly in and around the muciparous follicles or vesicles called the *Glandulæ Nabothi*, which eventually become enlarged and coalesce.

It would be most desirable were the doctrines delivered by these authors and Dr Ashwell perfectly well founded; for, assuredly, they would enable many a physician to give a more favourable prognosis than he can do in such cases, and they farther would tend to inspire confidence in the employment of therapeutic measures. We fear, however, that this is a view which the present state of pathological knowledge and therapeutic experience does not permit us to entertain. The facts hitherto ascertained, and the doctrines even taught by different observers, lead, we conceive, only to two conclusions; *first*, That there is a class of tumours incident to the uterus, which are not liable to proceed to the peculiar form of ulceration called *cancerous;* and, *secondly*, That there is another class which is liable to proceed to this sort of ulceration, whatever means be employed to prevent the occurrence or retard the progress of such a catastrophe. The former are the Fibrous or firm Tumour; the latter are the Skirrhous or Karkinomatous Tumour. It is a mere corollary from these conclusions to say, that in the former, remedies may be advantageously employed; in the latter, whatever remedies are employed, no favourable influence from them can be expected; and there is some reason to believe that the use of many of the remedies recommended is hurtful.

The following passage may be given as containing a general summary of the pathological doctrines of Dr Ashwell upon this subject.

> "Opinions of the nature of cancer must vary in accordance with the more or less extensive views which may be adopted. If it be believed that its materials exist in the blood, as well as in the component or molecular structure of organs, the locality only being undetermined, the affection must be without any other limit than that which includes the entire organisation. If, on the other hand, it be regarded as a disease of glandular structure exclusively, its ravages will be confined within narrower limits.
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> "By the disciples of Broussais, chronic inflammation is viewed as its essential condition. By others it is limited entirely to the consti-

tution, topical circumstances being considered not at all influential. It has been said to depend on depravation of the nervous fluid, on an enlarged and varicose condition of the veins; and by Mr Carmichael its origin has been ascribed to the generation of hydatids. These peculiar and exclusive views are certainly not calculated to inspire any confidence. Velpeau, Andral, Cruveilhier, Berard, and Carswell are decided advocates, with certain modifications, of the cancerous vitiation of the blood itself; and there is no doubt that carcinomatous and encephaloid matter has been found in the interior of both veins and arteries. I cannot, however, after a careful perusal of all the reported cases of this kind, discover any proof that these cancerous products existed in the blood or its vessels alone; in other words, independently of, or previously to, the development of the disease in the tissue of the solids. Dr Carswell does, notwithstanding, affirm, that the blood is the sole primary seat, and that he has seen cases where the venous blood alone was contaminated by the disease. Such instances have not fallen within my observation; and certainly, till the statement is supported by the fullest and most accurate records of the examples themselves, it will fail to command extensive assent. Dr Hodgkin, without explaining the method of their origin, contends for the existence of compound serous cysts as the basis of malignant structure. There are numerous corroborative illustrations of this opinion in the Museum of Guy's Hospital. Müller, the celebrated German pathologist, has apparently established the fact, that the microscopical elements of cancerous growths, and their mode of propagation, are not only similar to the growth and arrangement of benignant tumours, but of the natural structure of the fœtus. Hence he infers, that carcinoma is not a malignant or heterologous formation, forgetting, probably, as Dr Walshe acutely remarks, that such identity simply shews that the heterologous character is produced not by the nature, but by the mode of combination and arrangement of the ultimate physical elements of the diseased growth.

"From all which has yet been observed and settled as true, it may, I think, be assumed, that the most frequent primary locality of cancer is not in the blood, but in the molecular structure of organised tissues or parenchymata, and that the deposit of the morbid material is dependent on perverted nutrition or secretion."—(Pp. 380–382.)

To complete the information on this point, the following remarks on diagnosis deserve attention.

"The distinction between malignant affections of the uterus, and those of simple character, is not always easily made. There are cases of engorgement, hypertrophy, and induration, in which the finger introduced into the vagina discovers an increase of volume, either in the entire uterus, the cervix, or in the body only. Now as these are changes induced by cancer, and as there may be slight or very severe pain in all the affections, it is important to point out the diagnostic characters.

"Simple engorgement, hypertrophy, and induration, are less hard, of more uniform surface, often unnaturally warm, and tender on pressure, whatever part may be affected; while even in the early stages of cancer, the surface is irregular and rough, free from tenderness; and there is often a weight, coldness, and stony induration.

"In cancer, and the simpler affections already mentioned, there is a marked difference in the mucous membrane, covering the cervix. In the former it is of a dull white, or slightly gray, colour; in the latter it is much redder and more vascular, and often morbidly sensitive.

"Hypertrophy, or common induration, may affect either the body or cervix separately, or at the same time; but never in so isolated a form as to give rise to distinct and separated nodules of tuberculous induration, like carcinoma. Scirrhus develops itself very slowly; the former affections rapidly; frequently reaching a size in six or eight weeks, which scirrhus would require as many months to attain.

"Simple enlargements are generally easily cured by the means already pointed out; while scirrhus, in its earliest formation, requires a much longer period. Common induration is nearly stationary. Malignant disease, although slowly, is gradually progressive, and by affecting neighbouring tissues, transforms them; and sooner or later, —by their consolidation, destroys the natural mobility of the uterus.

"When softening and breaking down have occurred in the cancerous mass, it may at first be mistaken for simple ulceration of the cervix, for corroding ulcer, or for chancre. The history, the symptoms, and above all, the touch and the speculum will remove every perplexity. The peculiarities of carcinoma—its primary induration; its affection of surrounding parts; its fungoid growth in the seat of the ulceration; its peculiar pain; the accompanying immobility of the uterus; the fetor of the discharges; the hæmmorhages; the cachexia and emaciation; with many other particulars too well understood to require any mention, will prove sufficiently diagnostic."—(Pp. 382, 383.)

The difficulty in the diagnosis, and the possibility that chronic inflammation and its effects may be mistaken for skirrhus, always afford a sort of conditional provision for a modified opinion, more or less ground for hope, and more or less field for judicious medical treatment.

It must not be imagined, nevertheless, that though either skirrhus or karkinoma be pronounced incurable, and though in any given case, the state of the parts and the other symptoms may justify the inference, that the patient labours under Cancer of the uterus, therefore all medical advice and treatment is useless. For a good deal of both there is room even in cases apparently confirmed and hopeless. If the disease be in the early stage, it is always serviceable to have recourse to means calculated to abate local inflammation and irritation; and even in the advanced stage, much may be done to alleviate the sufferings of the patient. Dr Ashwell not only believes in the possibility of curing these tumours in the early stage, but lays down full directions for treatment, both prophylactic and curative; and at more advanced periods, he specifies the means to be adopted in order to alleviate pain, and contribute to the comfort of the patient. The grounds on which he proceeds, as far as they depend on the form and state of the disease, may be understood from the following passages.

"The muciparous glands, in the interior of the cervix, may be hard, and of the size of small shot, and pressure on them may induce pain, and yet, if the mucous membrane covering them be not ulcerated, a restoration to a healthy state may, by proper treatment, be fairly hoped for. I am aware that, in a slight degree, such a state of the muciparous glands may occasionally exist, as the result of irritation, induced by various causes, as painful and excessive intercourse, dysmenorrhœa, &c.; but the effect is then generally transient, and unaccompanied by the more permanent symptoms already mentioned. These little indurated glands are often associated with a hard and fissured state of the os, and an enlarged and hard cervix. The turgescence of the interior of the neck, and its deep flesh-colour, both within and externally, are well marked. The uterus is usually increased in bulk, and feels altogether thicker and more solid.

"If the vagina is at all knotted and indurated; if the cervix is united to the vagina by hardened mucous membrane, and cannot be moved freely; if the uterus generally is fixed and consolidated with the neighbouring organs, or if there be abrasion, softening, or commencing ulceration; then the case wears a very unfavourable, but not an entirely hopeless aspect, and a most cautious prognosis must be given."—(Pp. 385, 386.)

The great principles of treatment are, *first*, the removal of all sources of irritation; *secondly*, to arrest, by topical remedies, the further development of the poison, and to destroy or neutralise that which is already acting; and, *thirdly*, to sustain and increase the strength of the system, by judicious attention to the digestive functions, by suitable diet, pure air, and regulated exercise.

Upon all these heads the directions given are wise and sensible. We merely remark, that the use of mercury, which the author, in imitation of Dr Montgomery, recommends, appears not to be a judicious measure; for in all instances of skirrhous hardness and cancerous sores in which this mineral has been tried, it has never been known to do good, and has often been observed to produce unequivocal mischief.

As to the use of the narcotic drugs, which have been more or less strongly recommended, and generally used at all times—for instance, *belladonna*, *stramonium*, *hyoscyamus*, and aconite—he allows that cancer never has been cured by any of them; and all that they do is to relieve pain, and that in a very slight degree. He recommends, notwithstanding, that they should, with iodine, mercury, and iron, form part of the treatment, that is, part of the remedial agents. Of iodide of iron, in combination with arsenic and hemlock (*conium*), Dr Ashwell speaks in favourable terms.

Local bleeding by cupping, or by leeches applied to the *perinæum*, the *vulva*, or the *cervix uteri*, or by scarifications of the latter part, he considers as entitled to the confidence which has been reposed in them. The propriety of scarifications in well-established skirrhus or cancer of the uterus, we think, must be doubtful, as the irritation which they cause is often considerable. The bleeding by leeches applied as near to the parts as may be practicable, is a much more certain remedy, at least to effect the purpose of relieving inflammatory heat, pain, and swelling, and is followed by greatly less irritation.

The warm bath and the semicupium repeated as often as can be done without fatigue, Dr Ashwell also regards as a serviceable remedy for relieving local uneasiness and promoting sleep.

After some degree of relief has been obtained by the use of the means now mentioned, and the stony hardness of the muciparous glands has been diminished, Dr Ashwell thinks it may be proper to employ such agents as blisters, and setons, the topical application of iodine, the chlorides of zinc and mercury, and nitrate of silver. Of the propriety of employing bichloride of mercury, that is corrosive sublimate, he expresses apprehension, because it induces much pain, local heat, tension, and other marks of inflammation. Chloride of zinc, so much commended as an escharotic, he employed once ; but its application was attended with much pain, and followed by no good result in improving the surface, to which it had been applied. Of arsenic, sulphate of copper, and nitrate of mercury, he allows that he has no experience.

It would not be becoming in us, to speak in a more confident manner of the effects of these agents than does Dr Ashwell. But it may be observed, that the tendency at present, and for a long time in diseases of the kind now under consideration is rather to do too much, than to err in doing little or nothing. Almost every practitioner has his specific application, and very freely often he applies it. In proportion also as these applications are powerful and energetic, it seems that they are regarded as in the same degree useful. Amidst all this activity and restlessness, however, it is completely forgotten, that the structure of the parts so treated has undergone so great a change, that, if these agents act as escharotics, they merely destroy one unhealthy surface to make way for another to succeed in its place ; and if they exert the irritant action short of the escharotic, they aggravate the morbid and disorganised state of the parts. Dr Ashwell might perhaps have been usefully employed in cautioning his younger and less experienced brethren against the use of articles, which there is too much reason to believe are employed much more frequently than is necessary, and the refraining from the use of which altogether, would do no manner of harm.

The author next considers what is to be done in the advanced stage of Cancer of the Uterus. We are unfortunate in thinking that little can be done, except by means for keeping the parts as clean as possible, and diminishing the irritation of the discharges. We nevertheless strongly recommend to the reader the attentive perusal of the author's observations and instructions. The excision of the *os* or *cervix uteri* is an operation upon which we think the profession has already pronounced its opinion. It can be of no use to remove by a painful operation morbid growths, which will certainly be reproduced upon the cicatrix.

Next come the subjects of Simple Ulceration and Induration of the *Cervix* and *Os Uteri;* Female Gonorrhœa; Syphilitic Non-chancrous Ulcers, and the treatment required by each disorder; Corroding Ulcer of the Uterus; Cauliflower Excrescence of the Uterus; Occlusion and Rigidity of the *Os Uteri;* terminated by concluding observations.

The disease called Simple Ulceration and Induration of the Cervix is a peculiar and rather mysterious affection. In the young and unmarried it is rare. In the married and those bearing children it is not uncommon. It may appear either in the form of erosions of the mucous surface, chiefly confined to the epithelium; or in the form of a granular ulcer, that is an ulcer with distinct firm granulations; or in the form of chronic ulcer of the cervix, indolent in some respects, with the granulations more florid than in the second case, and considerable inequality of surface.

The causes of these forms of ulceration are supposed to be cold; insufficient clothing of the lower part of the person; wetting of the feet during menstruation; the improper use of strong astringent injections, bougies, and other foreign bodies; some vitiated state of the seminal fluid of the male; neglect of cleanliness, any cause, in short, which may act as an irritant to the *os uteri.*

The treatment consists in the observance of cleanliness, the application of leeches if there be much swelling; the recumbent posture; the hip bath, either saline or medicated with poppy or hemlock; spare diet; abstinence from sexual intercourse; and cauterization with the nitrate of silver. In slight cases astringent lotions of oxide or sulphate of zinc, are beneficial. The acid nitrate of mercury and the caustic potash (*potassa fusa*) are also employed. But Dr Ashwell thinks the nitrate of silver is the safest application.

Corroding Ulcer of the Cervix, is a disease which so closely resembles Cancer, that the diagnosis is often difficult, and must always be so, according to Dr Ashwell, unless the speculum be employed. He allows, however, that it is a disease more hopeful, that is, allowing the practitioner to entertain stronger expectations, that it may be cured than cancer. It is attended with the same tendency to bleeding, sometimes profuse enough, as in cancer; and the discharge is not less acrimonious and offensive. It evinces also the same tendency to the destruction of the parts on which it is seated, and the same disinclination, as it may be named, to assume anything like a healing process. It is, however, a disease which advances in general more slowly than Cancer does, and it seems to allow more easily of the application of remedies. Its surface when examined by the speculum is red, rough, and granular, with a distinct, generally sharp edge, marking its extent. Yet adjacent to the ulcer, the structure of the uterus is free from induration. This last circumstance constitutes a decided difference between Corroding Ulcer and Skirrhous or Cancerous Ulcer. This disease gradually distroys and, as it were, consumes away the neck of the womb, the body of the organ, as far as the fundus, and not unfrequently forms false passages between the bladder and the rectum.

Dr Ashwell thinks that it is impossible to doubt that the Corroding Ulcer is the result of inflammation; yet he informs us that he has been struck with its close resemblance to Lupus in internal parts. If Corroding Ulcer be really the effect of inflammation, it cannot be similar to Lupus. He admits, nevertheless, that to infer that the Corroding Ulcer is the effect of chronic inflammation and its consequences, is a more plausible opinion than that which ascribes the skirrhous deposit and cancer to chronic inflammation. On the other ~~it~~ hand, it must not be forgotten that several competent authorities regard Corroding Ulcer as closely allied to cancer.

The treatment of Corroding Ulcer is the same as that for cancer. The application of the solid nitrate of silver seems to possess more power than any other agent over the disease. But this is but small under any circumstances.

On the subject of the Cauliflower Excrescence, the information is full and correct. Little new, however, is made known regarding the nature of this disease. The disease evinces its malignant character by returning after the mass has been removed either by the knife, ligature, or caustic. On this point Mr Safford Lee expresses a difference of opinion. This gentleman regards the disease as void of malignancy, on the ground that if the whole mass of the disease be removed, it does not return; and he adds, that several cases are now on record where this disease has not returned after its entire removal.

"We find," says Mr Safford Lee, "that upon the question whether the cauliflower excrescence is or is not cancerous, good authorities are divided."

"Drs Gooch, Hooper, Davies, Ashwell, and Lee, regard it as truly cancerous; whilst others, as Drs Clarke, Burns, Simpson, and Walsh, consider it as a morbid tissue not necessarily of a malignant or carcinomatous nature. The principal argument used by the former practitioners is that of its liability to be reproduced after operation. But we find that if the whole of the disease be removed, it does not return.

"I agree with those who suppose that the cauliflower growth is not carcinomatous, and there are many circumstances to favour this presumption. In the first place the patient enjoys good health throughout the disease. In Anderson's case, No. 5, there was no pain like that of cancer; the disease was unnoticed until it had attained a considerable size, whereas, in cancer, it is almost the first and prominent symptom. There is no great absorption of fat, as in those who die of cancer (Clarke). Again, it occurs early in life: Sir C. Clarke has seen a case at the age of twenty. It is confined to a particular part, and does not involve other strictures: however, when the disease is of long standing, it may involve the upper part of the vagina: I have seen this in two cases. I think then that these reasons are sufficient to justify the opinion that these growths are not cancerous."—(P. 88.)

The description of the anatomical character of this tumour by Mr Safford Lee is entitled to attention.

"This tumour invariably grows upon the *os uteri*, that is, just within its lips, and is seldom found to extend into the uterine cavity. It commences by a number of small granulations or warty excrescences, which gradually increase, are firm to the touch, and divided into small portions, having many clefts passing down to their base, which, at the commencement, is as large as any other part of its structure; in fact, its appearance is that of a cauliflower.

"One peculiarity has been observed, namely that hardly a preparation of this disease can be found in any of the museums; because, always after death, the tumor which had formerly occupied the whole vagina, disappears, and on *post-mortem* examination, only a mass of shreds can be seen. If a ligature be applied to a tumor of this description, a mass does not always come away, but you find that it en-circles a few ligamentous shreds, which, previously to their stricture, had been so large as to cause great difficulty in applying it. I saw a singular case of this sort, where a surgeon had made several attempts to encircle a tumour of this description by a ligature, but the mass was so large, and the opening to the vagina so small, that it entirely prevented its application; at last the patient, worn out by the profuse watery discharge, died. I was present at the *post-mortem* examination, and we could only find a few shreds, but no trace of the tumour which had formerly filled the vagina. The same circumstance took place also in Case No. 5.

"The density of this morbid product varies: in some cases it is very friable and breaks down under the fingers; but in others it is hard and firm, although it may still disappear after death."—(P. 83.)

"Although the structure of the tumour is so vascular, it does not give rise to hæmorrhages: any external injury, however, produces bleeding, and it generally follows coition, examination, violent emotions of the mind, the acts of sneezing, coughing, and defœcation.

"This kind of tumor is very liable to be reproduced after its excision; the slightest portion of the disease, if left, gives rise to a large tumour. This fact suggests the necessity of *entire extirpation:* and from observation it may be laid down as a rule, that unless the part from which the morbid product grows is extirpated, the tumour will be reproduced.

"On examining a portion of the tumour taken away, in Anderson's case, the granulations appeared to be covered with a fine membrane, producing a shining appearance, and small vessels were distinguished ramifying over it. When a portion was squeezed between the fingers, the substance became pulpy.

"Under the microscope, these lobules were found to be covered individually by epithelial scales resembling those of the mucous membrane; and each was composed of nucleated cells, with here and there a bloodvessel ramifying on it, but the tumour was not apparently vascular. The edge of the lobules, with epithelial scales, appeared as if impacted one upon another; beneath which, from its circumference, where the cells were much compressed to its centre, cells became gradually developed. There was no appearance of fibrous tissue, nor any of the caudate cells indicating cancer. This, then, was the result of a careful examination of a part of this tumour removed during life by Dr Richard Quain and myself. The following is a description of a portion examined in the same way after death:—When a piece of the tumour, the only remains of which were in small detached clusters, was taken and placed in water, it appeared to be made up of a number of villi, apparently attached to a central substance of more firm consistence. It was composed of nucleated cells of large size, some circular, some oval, and others elongated oval; these contained a quantity of granular matter and a well-defined nucleus, which appeared to contain a cavity filled with a quantity of granular matter. The two together had the appearance of a cell within a cell, or a compound cell. These cells were connected by fine filaments like cellular filaments. From this examination we conclude that the tumour is composed entirely of cells, and that these are covered by an epithelial membrane; also, that it was of simple structure, and not malignant."—(Pp. 84, 85.)

Mr Safford Lee further quotes, from the microscopical examination of an instance of the disease, by Dr Anderson of Glasgow, various facts which tend to confirm the correctness of his own observations. According to the account given by Dr Anderson, there is no structure resembling what Sir Charles Clarke has described as a general investing vascular membrane. In a portion of the vascular membrane, which consists of several layers, he detected a fine fibrous structure of great delicacy, abounding in blood corpuscles and cells, to the presence of which great part of its apparent thickness seemed owing. The course and distribution of the capillary vessels could not be distinctly traced. Besides the corpuscles which retained their form, there were others apparently undergoing various changes, presenting every variety of shape, and mixed with nucleated cells, of different aspects.

> "The structure of the cauliflower excrescence was now manifest. The basis is a membrane of extreme tenuity, ramifying most complexedly, amply supplied with blood, and possessing the power of forming from that blood a whitish cell substance, which is deposited on a layer around it. Hence each portion of the membrane forms, after maceration, a kind of lobule or flat villus; but in the recent state they adhere closely together, so as to give the whole tumour a nodulated aspect."—(P. 86.)

The great evils of this growth are the constant discharge from its surface, which weakens the patient, and the occasional hæmorrhages to which, when injured, it is liable to give rise. Hæmorrhage may take place after sexual intercourse, when the bowels are emptied, upon manual examination, after coughing, sneezing, and similar actions. The continuance of the tumour in this manner invariably impairs much the health and strength of the patient, who becomes feeble, emaciated, dyspeptic, anæmic, and hysterical.

Dr Ashwell thinks that of ultimate recovery there is no chance; and the history of most cases of Cauliflower Excrescence furnishes too decided a confirmation that this opinion is well founded. Mr Safford Lee, on the contrary, has been led to conclude that, if the whole tumour be removed, it does not return, and the patient is preserved. It is not difficult to perceive that this statement is conditional; and Dr Ashwell justly expresses the apprehension, that the cases in which this condition can be observed, are few indeed. In truth, whether it be that, in the majority of authentic cases of cauliflower excrescence, it has been impracticable to remove completely the tumour, or whether it be that even when eradicated, it returns, certain it is, in most of the patients the disease has terminated life.

The means of treatment consist in the operative and the palliative.

The former are, either to enclose the base of the tumour in a ligature, or to employ caustic to the excrescence, or to remove, by incision, the *cervix* of the uterus to which the growth is attached.

The latter consist in rest in the recumbent posture, abstinence from sexual intercourse, rather moderate unstimulating than nutritious diet, the use of gentle aperients or *enemata*, the salt-water douche-bath over the back and shoulders, abstraction of blood, by cupping or the application of leeches, from the loins, hypogastrium, or perinæum, and the cold alum hip-bath, or the moderate use of astringent injections.

Under the cautious and judicious employment of these measures, sometimes separately, sometimes conjointly, as the circumstances and strength of the patient may indicate, life has been prolonged for years after the appearance of this disease, with considerable comfort. The patient has much in her own power, by practising self-denial in all modes.

The last subject treated in this chapter is Occlusion and Rigidity of the *Os Uteri.*

Entire Closure, and so great a degree of Rigidity, as to preclude the birth of a child, without laceration, if aid be not given, are rare. Two cases of this kind, nevertheless, have occurred in the practice of Dr Ashwell. Dr Gooch gives one among many that had occurred to him ; one occurred to Dr Wallace of London ; and several examples of complete closure and great rigidity are recorded by Dr Naegele of Heidelberg.

It is well known, that, normally, the orifice of the uterus may be very small. In other instances, instead of a transverse chink, there is merely a minute circular aperture. In either of these cases, a very moderate amount of local inflammation succeeding to conception, or taking place in any way, might so perfectly obliterate the orifice, as to render parturition impracticable. There is no necessity for organic change in this instance ; that is, change of structure in the cervix. It is probably merely adherent, in consequence of previous swelling and inflammation.

It has been indeed argued, that complete closure is an impossible occurrence, and that the apparent absence of *os uteri* is owing to obliquity, or some similar malposition of the womb. Perhaps this is the case. But still the practical difficulty and impediment to parturition is the same ; and it is equally necessary that this obstacle be removed, in order to prevent laceration either of the neck or the body of the uterus.

Dr Ashwell adduces conclusive evidence to shew that the most safe and serviceable practice is incision of the *os uteri;* that it is greatly more eligible than any attempts at mechanical dilatation; and that this operation ought to be performed as soon as the attendant and his adviser have satisfied themselves that either the *os* is adherent and occluded, or that the surrounding tissue is so rigid that it is impossible to expect the natural efforts to be adequate to the exclusion of the fœtus.

A Polypus, Mr Lee defines to be a tumour generally pyriform in shape, growing from the inner surface of the uterus, attached by a stalk or peduncle; smooth, hard, and insensible, and causing from time to time profuse and frequent hæmorrhages.

The seat of these bodies varies. Though they may spring from any point of the internal surface, their most usual source is the posterior surface of the cavity and fundus of the uterus, and near the openings of the Fallopian tubes. This, M. Lisfranc states, he found to be the case in forty-three among sixty cases; and the inference is confirmed by the observations of Mr Lee.

In size, they are most variable, ranging from the bulk of a small millet seed to the size of an orange or the fœtal head.

No age is entirely free from the occurrence of polypoid growths. They have taken place at so early a period as two years, eight years, and fifteen years. But the most usual time for their formation is the space between the thirtieth and fortieth year. The following table, from Malgaigne, shews the respective ages at which fifty-one instances of polypus of the uterus took place.

			Years.
4	cases occurred	between	26 and 30
20	,,	,,	30 and 40
16	,,	,,	40 and 50
4	,,	,,	5 and 60
3	,,	,,	60 and 70
4	,,	,,	70 and 74
51			

That is to say, two-fifths between the age of thirty and forty; a little less than one-third between forty and fifty; and about one-twelfth at all other periods of life.

The causes of these growths are involved in obscurity.

What the author terms unnatural sexual excitement and irritation, has been by many considered to be a direct and frequent cause of the formation of polypi; and Chaussier is said to have attempted to prove this inference experimentally by inducing irritation in the mucous membrane of the uterus. Dr Lever, on the other hand, finds that the disease is more frequent in the unmarried than in the married in the proportion of seven to three.

The diagnosis is seldom difficult. In simple cases Dr Ashwell observes it next to impossible not to recognise a firm, insensible, and bulbous growth having a distinct stalk, and either embraced by the *cervix uteri*, or traceable to one of the lips of the *os uteri*. Such a body cannot be any thing but a polypus.

The states and diseases with which polypus is most liable to be confounded, are *Inversio uteri*, *Prolapsus*, hard tumours, fibrous or skirrhous, cauliflower excrescence, and it is said pregnancy.

Of these the first is the most likely condition for which polypus may be mistaken, and which is liable to be mistaken for polypus. With care and attention, however, it is easy to draw the distinction between the two. *Inversio* almost constantly follows delivery, and there is some time after considerable sensibility of the tumour.

From hard tumours whether fibrous or skirrhous, it is in general easy to distinguish polypus. The latter has one or more stalks; the former are either hard in the uterus, or form part of the organ.

From cauliflower excrescence it is easily distinguished by the absence of the granular surface, and the general structure.

The most constant symptom, and the most detrimental effect of polypus is the frequent and considerable bleedings to which its presence gives rise. Whenever, therefore, a female has hæmorrhage frequently recurring, examination should without delay be made, in order to ascertain what may be the cause. If the presence of a Polypus is recognised, the course of procedure is simple and clear.

"For practical purposes, it would be sufficient to divide these growths into the *hard* and *soft* polypi; but our improved pathology demands greater precision. The most common of all is the *fibrous* polypus, and it certainly far exceeds in frequency any other kind.

"Of *soft* polypi there is a variety, and their different designations by authors prove that between them there must be considerable pathological distinction. Thus the terms *glandular*, *vesicular*, *mucous*, and *cellular*, are but so many attempts to characterise a polypus softer, slimy, and more vascular than the fibrous species. There is also a peculiar polypoid growth, originating from disease of the glandulæ nabothi of the cervix, which is pediculated, of moderate size, lobulated in form, and somewhat resembling nasal polypi. This is generally swollen, and filled with a viscuous fluid; but polypi are not always round and pediculated, being sometimes cylindrical, without any separation into stem and bulb. Lisfranc, following M. Malgaigne, enumerates five varieties:—The *vascular*, the *cellulo-vascular*, that consisting of *hypertrophy of the tissue of the uterus*, the *moliform*, and the *fibrous*.

The *fibrous* polypi vary considerably in several important particulars. Some are hard and tuberose; and these, when complicated with fibrous growths in other parts of the uterus, may be regarded as the same disease, suspended from larger masses by the formation of a pedicle, and by descent through the os into the vagina. In such cases, bleeding, leucorrhœal, and especially purulent discharges, but more rarely ulceration or breaking down of the separated portion will occur, when its removal by the knife or ligature is called for. Not unfrequently the fibrous polypus is *fleshy* and *red*, resembling the larger muscles; or, it may be hard and firm, of a pale yellow, grey, or even white colour, but slightly vascular. Fibrous polypi differ much in their density; sometimes they are of soft consistence, or partially hollow, containing distinct bloodvessels, or cysts filled with grumous blood, or as in one or two instances, gelatinous matter and hair, or fat with hair. Their external covering is generally derived from the mucous membrane of the uterine cavity; and if their base, instead of being extensive and hard, is superficial, and immediately underneath the lining membrane, they quickly become pediculated, and from a very early period of their existence they are pendulous. Internally, fibrous polypi are made up of a tissue nearly resembling that of the uterus itself. Polypi have generally been regarded as devoid of nerves; this is scarcely correct. We know that the cervix of the uterus is not highly sensitive, being supplied from the great nerve of organic life; a polypus probably derives its slight sensibility from the same source."—(Pp. 495–497.)

Mr Safford Lee gives a division of SOFT POLYPI, differing from the ordinary FIRM POLYPI usually met with; and these he distinguishes into six varieties: 1. Vesicular Polypi; 2. Polypi from the enlargement of the Nabothian Glands; 3. Fibro-Cellular Polypi; 4. Cellulo-Vascular Polypi; 5. Mucous Polypi; and, 6. The Channelled Polypi of the *cervix*.

1. The Vesicular Polypi are soft tumours attached by footstalks, covered by a fine membrane, and composed of a number of small mixed vesicles or cells, which contain in them transparent fluid. The whole mass is supported by a thin fibrous tissue. These polypi are of a dirty white colour, and sometimes present a yellow tinge. It has been supposed

that these polypi are produced by the diseased state of the uterine glands, and that the same action which produces polypi of the glands of Naboth in the *cervix*, causes the production of these bodies from the uterine follicles.

2. The Glands of Naboth at the *os* and *cervix uteri* are liable to enlargement, producing translucent cystiform bodies containing a clear limpid fluid. This may be regarded as the commencement of polypi at the *os* and *cervix*. When once begun, they continue to increase in size; one, two, or three being larger, or growing more rapidly than the others. As they increase, the neck or foot-stalk becomes elongated, a result probably owing to the weight and traction of the head or bulb. These are not an uncommon sort of polypi.

3. Fibro-cellular Polypi. These, next to the polypoid tumour, are most commonly met with within the uterus. Their name expresses the nature of their structure. Dr Oldham observes that in general the void spaces or cells are the orifices of truncated and divided veins, and the tumour may not improperly be termed a Venous Tumour, the thin and delicate veins being surrounded by an unstriped fibre, closely resembling, if not identical with, the muscular tissue of the uterus.

4. The Cellulo-Vascular Polypi are seen most usually in the form of small red tumours lying between the *os uteri*, and much resembling the excrescences at the orifice of the urethra. In some severe instances they assume the appearance of fungous growths round the *os uteri*.

5. The Mucous Polypi appear to consist in elongations of the mucous structure itself. Their usual site is the cavity and the neck of the womb.

6. The Channelled Polypus of the Cervix is a rare form of tumour, and its peculiar character can be ascertained only after death, or its extraction. This was first described as a distinct species by Dr Oldham, who found that the foot-stalk contained several small orifices, most of which were vascular trunks; and the outer surface of the stalk had some concentric rings rather elevated above the surface, and was full of small openings, from some of which blood flowed when the tumour was pressed. "The large valvular orifices were found to lead into the interior of the Polypus, dilating into channels which were lined by a thin rugous membrane inflected from that which covered the Polypus. Other channels led out in different points from a large one; and so the growth was traversed by these channels, which were all more or less full of mucus."

Of all these tumours, that is, the Polypus and the Polypoid, the treatment is one and the same. Removal of the tumour either by excision or by ligature, are the only means adequate to the eradication of the disease, and the safety of the patient.

Both authors consider carefully the comparative merits of the two modes of operation, and the peculiar circumstances which, in each individual case or class of cases, indicate which method is most applicable.

A considerable degree of preference for excision has been of late years evinced by many practitioners, principally from the success which has attended the operation in the hands of Dupuytren, Lisfranc, Velpeau, and other eminent surgeons. Its advantages are, that the operator by one proceeding, and at one time, delivers his patient from a troublesome, dangerous, loathsome, and exhausting disease, and that no animal mass is left within the vagina to undergo decomposition, and become for some days a source of offence and discomfort, and, it is added, of probable danger, from absorption of putrescent matter. The disadvantage is the apprehension, and, it is said, the danger of hæmorrhage, which in a few cases has been considerable, and in one or two fatal.

With English practitioners the most usual practice hitherto has been the use of the ligature, partly because it has been found perfectly adequate to the cure of the disease, and partly, because to patients it has a less formidable appearance than the employment of cutting instruments. Dr Ashwell is evidently strongly disposed in favour of the ligature applied by means of the double cannula, after the method of Levret, or rather Niessen, as improved by Dr Gooch, and with the addition of the silver windlass of Laundy. The material which he employs to effect deligation is common strong whipcord. Neither silver wire, nor any similar attenuated matter answers for ligature, for the reason that such ligatures cut, instead of strangling the neck or foot-stalk of the tumour.

Dr Ashwell speaks from the experience of having tried both the method by excision and that by ligature. In certain instances, in which there is reason to apprehend much hæmorrhage, providing excision were adopted, he recommends, as the safe course, that the operator should first inclose the neck or foot-stalk within a ligature, and either immediately, or in a few hours after, excise the tumour below the ligature. This course he has successfully adopted in several instances.

Several interesting cases are given, illustrating the effects of the disease and the mode of treatment. That related in No. 78 is instructive in this respect, that, with a polypus, or polypoid tumour, projecting from the *os uteri*, there was associated a fibrous tumour above, in the uterine wall. The inclosure of the polypus in a ligature was followed by death of the tumour in the uterine wall; and the result was, that though, in the mean time, alarming symptoms, followed by almost mortal fainting, took place, yet the patient made a speedy and perfect recovery.

The next subjects are Malignant Growths and Ulcerations of the Uterine Cavity; Enkephaloma of the Uterus; Ulceration of the Mucous Lining of the Uterus; Physometra, or the Presence of Air within the Uterus; Hydrometra, or Dropsy of the Uterus; Abscess in the Walls of the Uterus; Uterine Moles; Vesicular Moles, or Hydatids; Osseous and Calcareous Tumours of the Uterus; Phlebolites; Atrophy and Hypertrophy of the Uterus.

Under the head of Hydrometra, or Dropsy of the Uterus, an affection attended by a profuse serous, or sero-albuminous discharge, is recorded an example of that malady at p. 526; interesting by reason of its being well marked, and the complete recovery of the patient.

Under the head of Abscess in the Walls of the Uterus, are given two very melancholy cases, in which the posterior wall of the uterus was entirely destroyed, and this organ and the rectum were thrown into one communicating cavity, both terminating fatally.

The Sixth Chapter is devoted to the subject of Displacements of the Uterus, in the following order: Relaxation; Prolapsus and Procidentia; Inversion, Anteversion, and Anteflexion; Retroversion and Retroflexion; and Hernia of the Unimpregnated Uterus. Of these different forms of displacement, Dr Ashwell gives correct and instructive descriptions; and, in explaining the treatment adapted to each, his observations and directions are instructive.

At the close of the chapter, the author makes some observations which will no doubt be perused with interest, as indications of the state of obstetrical Medicine and Surgery in the middle of the nineteenth century.

Dr Ashwell differs from some of the authorities of the present time as to the alleged frequency of the displacements known by the names of retroversion and retroflexion, anteversion, and anteflexion, and as to the possibility of these maladies being mistaken for fibrous or cancerous tumours. Neither does he concede to the instrument, by which their presence and degree has been supposed to be capable of being ascertained, the advantage, which has been claimed for it.

> "I cannot avoid thinking that this uterine sound not only detects, but makes many of these supposed displacements. All practical men know, that the uterus varies naturally in its position, in its degrees of mobility and immobility, and in the influence exerted upon it, as to position, by a loaded or empty rectum or bladder: and it must be kept in view, that the curve of this steel bougie may not be the curve of the uterus; and if, therefore, it is to be introduced at all (and I wish it were far less frequently so), the normal position of the organ, thus spiked, must be made to follow the curve of the iron instrument, entering, and thus unnecessarily intruding upon its cavity.
>
> "If we contrast the symptoms of these supposed cases with the symptoms of real retroversion, retroflexion, and anteversion, the difference is very striking, and cannot fail to induce the conclusion, that the uterine sound, an unusual degree of mobility of the uterus, and a too slight acquaintance with the normal varieties of the curve of the womb, have led to great error as to the frequency of these truly rare maladies.
>
> "The uterine sound and its twin instrument, invented for permanently fixing a piece of ivory inside the uterus, are not harmless, but when used really do mischief.
>
> "Two cases have recently come under my care, and I have heard of more, where the results arising from the use of this instrument have been very serious. Looking at it pathologically, I can scarcely imagine any thing better devised for inducing disease. According to this practice, a piece of ivory, two inches and a half long, is to be introduced into the uterine cavity, and its bearing must of necessity be on some part of the lining membrane, a surface ill adapted to support the pressure for two or three months together of such an instrument. The consequences may be supposed. One of the patients from whom, after a long and distressing journey, I removed this ivory one-pronged fork, told me she had never been free from pain since its introduction. In addition, it had produced frequent and intense sexual excitement, preventing sleep for many nights together, and had kept up constant leucorrhœal discharge. In the other example, during the two months this instrument had been worn, the sufferer, in addition to the previous complaints, had never been free from sanguineous discharge, lumbar pain, and frequent desire to micturate. In both, the speculum shewed that abrasion of the os and the ostium vaginæ had resulted from the use of the so-called uterine supporter."—(Pp. 635, 636.)

It was our expectation to have concluded in the present article what was requisite to say on these works and their subjects. But we must not further prolong these observations. The subject of Organic Diseases of the Ovaries follows; and it is greatly too important to be introduced at the present time. In both works, and particularly in that of Mr Safford Lee, the information on the Diseases of the Ovaries and the treatment proposed for their cure, is full, accurate, and highly practical. In the mean time we must be satisfied with referring our readers for information on the statistics and operative department of the subject, to the abstract given in a previous part of this number, of the elaborate analysis of Dr Lee, which has been prepared subsequently to the publication of the work of Mr Safford Lee.

ART. III.—*Sketches of Brazil; including New Views of Tropical and European Fever, with Remarks on a Premature Decay of the System, incident to Europeans on their Return from Hot Climates.* By ROBERT DUNDAS, M.D., Physician to the Northern Hospital, Liverpool; formerly Acting Surgeon to Her Majesty's 60th Regiment; and for twenty-three years Medical Superintendent of the British Hospital, Bahia. London, 1852. Post 8vo. Pp. 449.

DR ROBERT DUNDAS has been five years in the army, in different quarters of the globe, and twenty-three years at Bahia, in Brazil, where he held the office of medical superintendent of the British Hospital in that port. He has accordingly beheld a good amount of service, medical and general; he has observed much; and he has acquired knowledge. He returned to England some years ago, and found, prevalent everywhere, erroneous notions upon the causes, the nature, and the treatment of various diseases, especially fever. These erroneous notions he has undertaken to rectify, where they admit, to remove entirely where they admit not, of rectification. With the object now stated in view, Dr Dundas delivered nine lectures at the Northern Hospital in Liverpool in the beginning of 1852; and these lectures, preceded by an introduction, he now publishes, with the view, as we understand, of diffusing more extensively the information and instructions which they contain.

In the introduction, Dr Dundas undertakes to prove, that the fevers of hot climates, and those of temperate and cold countries, are quite the same in origin and nature, and require of course the same treatment. The peculiar short fever, which appeared in the spring and summer of 1843 in Edinburgh, seldom exceeding five days in duration, Dr Dundas regards as quite the same as the fevers observed to arise in warm climates, and in districts which are supposed to be productive of endemic fever. This fever was distinguished also by presenting what were called relapses or returns of the symptoms, and which by some might be regarded as indications, that it was of an intermittent character. He maintains also, on the authority of some of the observers, that it is not different from typhus. Thus, remittent endemic fever, as it takes place in the West Indies and on the African coast, typhus, as it appears in the Cowgate and Grassmarket of Edinburgh, and the short relapsing fever of Edinburgh, of the spring and summer of 1843, and the subsequent epidemic of 1849, are all the same diseases. As a considerable number of the papers describing these fevers, in Edinburgh and Glasgow, appeared in the pages of this Journal,—a sufficient number to furnish evidence on these questions,—it is unnecessary to occupy time in investigating the question.

The greater number of the distinctions attempted to be established between these fevers, the typhus, the typhoid, and the relapsing, are either trivial or visionary, and cannot be verified at the bedside of the patient. The character of the eruption as a diagnostic mark, is entirely fallacious. It is singular the author does not add Endemic Fever. But for this he compensates, by informing the reader, that in the break-bone fever which prevailed at Charleston in the summer and autumn of 1850, and which is the endemic or telluric fever of the Southern States, Dr Wragg represents the eruption as " not peculiar in character, but varying almost infinitely."

All the differences, in short, represented to subsist between Typhus, Typhoid, and Relapsing Fever,—and we suppose Endemic ought to be added,—are not more considerable than may be rationally accounted for by individual peculiarities,—difference of season and climate, modes of living, the sanitary condition of the population attacked, with various other moral and physical influences.

One point, however, may be here recalled. The mortality in the first visitation of the relapsing fever was, excluding cases of typhus, one in sixty-three cases, or five deaths among three hundred and fifteen patients. If the typhus cases be added, the mortality is still not more than one in eighteen cases, or seventeen deaths in three hundred and fifteen patients.

The mortality in Typhus Fever varies in different places, from one in four or five, to one in eight persons.

Dr Dundas disapproves strongly of the system of depletion and catharsis in fevers; and maintains that the physician ought, without one hour's delay, to have recourse to quinine as the most certain and most powerful febrifuge agent.

It will be admitted that Dr Dundas has simplified in a remarkable degree the etiology and treatment of fevers.

In the first lecture, after some preliminary remarks on the advantage of extended observation in the study of disease, the author enters upon the consideration of the gouty diathesis, as the great precursor of decay in Europeans returning from the tropics. Dr Dundas represents gout to be rare in Brazil, though the inhabitants indulge freely in what are called the pleasures of the table. Three severe cases, however, he met with; and in all there was unequivocal evidence of derangement in the renal secretion. In ordinary circumstances in Brazil, he represents the exemption from gout—which he states to be the fact—to depend, not upon the diet being less azotised, but upon the activity of the skin, through which, he maintains, very sensible amounts of lactic and uric acids are removed from the system.

This inference, he thinks, is fully confirmed by what is observed to take place in many persons who, after spending a considerable portion of life in warm and tropical countries, return afterwards to the colder climate of Europe and Great Britain. These persons, he remarks, begin, after a short residence in their native land, to exhibit symptoms of decay. They present symptoms indicating the presence of atonic or anomalous gout, or of rheumatic gout. These symptoms may never have appeared before. But now, in consequence of the action of the skin being impaired and repressed, by the influence of lower temperature, and the long duration of cold weather, the kidneys are called more decidedly into action. To this action, however, he argues, these organs are now inadequate, and hence approach gout, simple, or complicated with rheumatism,—disposing to Apoplexy, Paralysis, Disease of the Genito-Urinary System, Functional and Organic Cardiac Disease, or anomalous derangements of the Digestive Functions.

The objection to this catalogue of evils is, that they are not very methodically arranged. Most observers in this country will be of opinion, that gout and rheumatic gout do not give rise either to derangements of the digestive functions, or disease of the genito-urinary system; but that the derangement in the digestive functions arising from long-continued habits of indulgence, and probably the disorder in the genito-urinary system are the antecedents and the causes of the gout and all the evils which it brings in its train.

Dr Dundas has ascertained by inspection after death, the fact that, lengthened residence in tropical countries, produces upon the structure of the kidneys most hurtful changes.

In five cases among nine, he informs us, after a lengthened residence of from sixteen to nineteen years, he found Atrophy in a greater or smaller degree to have taken place in both kidneys. In one case only one kidney was affected. In four cases, the organs were firmer than natural, but the intimate structure was apparently normal. One was softer and more flaccid, and the remaining four presented no appreciable change from the healthy consistence. In none had the form of the organ undergone any change. In all the nine cases the cortical substance was decidedly paler than natural; and in those atrophied, the change was limited to this tissue, the tubular cones being more distinct and injected than is usual in the healthy kidney of the European.

In none of the subjects of these nine cases had symptoms of urinary disorder been manifested during life; all died from diseases unconnected with the kidneys; namely, two from accidents, one from pneumonia, one from compound fracture of both lower extremities, one from acute bronchitis, one from apoplexy, two from dysentery, and one from rupture of the aorta. The duration of the diseases ranged from one to twenty-three days. All the patients were males from thirty-four to fifty-six years of age.

The author afterwards allows (p. 60), that the condition here described, can scarcely be denominated disease. It is, consequently, not easy to see, why it is noticed as a potential cause in terminating life, in the persons in whom it was found.

Actual diseases of the kidney, he also admits, are of much less frequent occurrence than diseases of any of the other important organs of the body. The granular kidney of Dr Bright he has occasionally seen.

Dr Dundas further maintains, notwithstanding the admissions now made, that the disorders and complaints of these tropical *revenants* are frequently misunderstood by a numerous class of the profession in this country. The blue pills, the alkalies, the salines, the tonics, and the purges with which the patient is treated, while the essential nature of his malady, namely, this shadowy disorder of the kidney, is either overlooked or unsuspected,—all testify, he says, to this truth. The liver, the stomach, the spleen, the whole associated chylopoietic viscera, are in turn condemned and assailed with the formidable and multitudinous batteries of the pharmacopœia and the materia medica. "How judiciously this is done," he adds," "in some cases, is shewn by perusing some of the prescriptions of fashionable and well-employed practitioners." —(P. 54.)

Dr Dundas, therefore, consistently enough denounces the employment of medicine for such patients, and recommends to them due and active exercise, mental and corporeal, and strict attention to diet and regimen.

In the second lecture the author returns to this non-actual disease of the kidney, and gives in detail the hygienic rules which he conceives should be observed by the exotised tropical valetudinary. These rules contain nothing particularly new to the medical reader. One passage may deserve the attention of the Chancellor of the Exchequer,—and his clients the wine-merchants. In speaking of the use of wine, Dr Dundas delivers himself in the following terms.

> "I would altogether exclude the trash commonly served up even at the tables of the wealthy as sound sherry; but which in truth is perfectly innocent of the slightest connection with the district of Xeres, being usually a mixture of Cape, Malaga, and Mediterranean vintages, to which are too often added nefarious compounds of home manufacture. Good sherry, one of the rarest of wines, may of course be allowed; but good Madeira or old Port is equally beneficial, or sometimes more advantageous. The sherry conceit is indeed a mere fashionable whim, supported by partial and imperfect chemical views, in which the play of vital affinities is completely excluded. My experience, which has not been limited, has satisfied me, that where an acid tendency existed, it could be ascribed with more truth to other causes than the mere chemical character of the wines. Of all wines, however, none equals Bourdeaux of the best quality. I do not mean that extravagantly high-priced wine, served up at private tables and the fashionable clubs, which is in truth like the sherry alluded to, a product of the manipulations of the wine manufacturer, and exceedingly prejudicial to a weakened stomach. Good Bourdeaux wine is the most healthful of all wines, and affords the greatest amount of beneficial stimulus with the least amount of injury to the human constitution. Moreover, with reference to gout, that malady is notoriously least prevalent in those countries where claret is the ordinary beverage. The notion that the climate of England demands a stronger or more stimulating wine, I hold to be, among certain popular fallacies, originating in prejudice rather than in reason, and which the introduction of sounder views in political economy, as well as in physic, will probably ere long dispel. In advocating the substitution of the lighter wines of France for the strong bodied and heating wines of Spain and Portugal, we afford another instance of the sacrifice of professional gain to the public welfare; as I am fully persuaded, that, if this change were generally adopted by the higher classes in this country, the favourable results to their health and purses would far exceed the most sanguine expectations."—(Pp. 85, 87.)

It must be admitted that it is not a matter of doubt, that a large amount of not very safe liquor is sold in this country under the name of sherry; but it must be added that the same applies in an equal, and perhaps a stronger degree, to Madeira and Port wine. The practice of manufacturing wine has been prevalent in this country since the days of Addison 1709–10;* and, in the time of Pennant, we have the authority of that gossiping peripatetic observer for the fact, that the business was then conducted in undiminished activity.* It has also long been said, that much more Port and sherry are sold in this country than are grown in the countries where they are represented to be produced; so that the expectation of obtaining such a thing as real genuine wine, unless at a very high price, or imported by known channels, must be altogether delusive.

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* *Tatler*, No. 131, February 9, 1709–1710.—Trial of the Wine Brewers.

This is not the whole amount of the evil. These liquors, in the state in which they are sold and consumed, are not wines but brandies; and the idea of applying to them the name of wine is confined to the inhabitants of this and similar countries, in which the taste for these strong spirituous compounds is encouraged.

The chief cause of all this mischief is, the high duty imposed upon these liquors. Each gallon of wine pays to the customs five shillings and ninepence, or eleven shillings and sixpence per dozen, which in the wine merchant's mode of accounting, is twelve shillings. This duty acts as a prohibition to the general use of good wine, and as a great encouragement to the manufacture and to the sale of factitious and compounded liquors. It is evident that, when wine is offered at such prices as twenty-five and thirty shillings the dozen, the article cannot be pure or genuine, because such a price supposes that the value of it is thirteen shillings and eighteen shillings each dozen. Yet such is the ignorance prevalent on this subject, that the great demand is for low-priced wines; and consequently the object is not so much to sell good wine as a large amount of inferior wine. It may be said, without much risk of error, that for one dozen of wine sold at three pounds, and below that price, little good Port or Sherry can be procured, ten dozens are sold at thirty shillings, and between that and thirty-six. If the profit is only the same on each quality, it is not difficult to see that it is the interest of the wine-merchant rather to sell the cheap article, however inferior and hurtful, than the expensive and costly one. Not long ago, when Mr Shaw, a considerable and intelligent wine-merchant in London, in a very sensible letter, urged the necessity of a reduction of the wine-duty, the wine-merchants were in the greatest alarm, and expressed distinctly enough their desire, that there should be no change. The reason is not difficult to understand. In the present state of the wine-duties, it is greatly more profitable to sell low-priced wines than those that are high-priced. On the other hand, it would not be difficult to shew, that the high-priced are really the cheapest, as they are the safest.

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* Some Account of London. By Thomas Pennant. London, 1790. 4to, pp. 32, 33.

The proposition made by Dr Dundas, to substitute the French wines for the spirituous compounds of the Oporto Wine Company and the London Wine-makers, has been often before made, and always without success; partly because the high duty renders it unprofitable to import from France any but the highest qualities of wine, and partly because our legislators and Chancellors of the Exchequer cannot be made to understand, that it is much more safe, as well as more profitable, to buy and sell with a nation, than to be on terms of hostility and indifference with them. The powers of France as a wine-growing country are almost unlimited; practically speaking they are unlimited; and were the duties reduced so much, as to open the market in this country, the French proprietors and cultivators would, in the course of three or four seasons, furnish any quantity required. From the present state of the demand and consumption in this country, they furnish only a moderate supply; because, were it much or suddenly increased beyond the demand, the price would fall so much, that this department of rural industry would be greatly injured, and many of those engaged in it would suffer temporary ruin. If, on the other hand, the duty were considerably reduced, it might cause a small reduction in the revenue for one or two seasons; but this would be followed by a permanent and steady increase in the course of two or three seasons. At present, the great uses of the duty are, not to furnish revenue, but to prohibit consumption, and consequently to facilitate and encourage the domestic manufacturer and compounding.

We have been inadvertently drawn into a disquisition on political economy, in consequence of the peculiar bearings of the present state of the wine-trade on the health of the public. It is impossible to doubt, that much injury is done to the health of the natives of Great Britain by the consumption of spirituous liquors, as whisky, gin, and brandy, by those who cannot pay for wine; and by the consumption of the spirituous wines of Portugal, and the compounds of the wine manufacturers, by those who cannot pay for genuine wine, either from France or the Rhine provinces. But Chancellors of the Exchequer find it more easy to allow the people to proceed in this system of self-poisoning, and creating patients for physicians, than to make a single step in the right direction; and every attempt made by any other person has been either so unfortunate, or done under such injudicious patronage, that, instead of accelerating and facilitating the process of alleviation, they have only thrown difficulties in the way.

We believe it to be altogether chimerical to expect that any beneficial change will be made in this matter, in order to improve the habits and promote the health of the present generation. But we may take this opportunity of saying, that the people have the matter in their own power. The safest and the most economic course is, to discard wine entirely, and to drink nothing but water. It is quite a delusion to imagine that either sherry, or claret, or any other vinous liquor, is either useful or necessary to the dyspeptic. It has been proved repeatedly, that the use of all these liquors is most hurtful to digestion, and that nothing is so safe, as nothing is so economical, as pure water, either cold or tepid. To all invalids, indeed, and especially the gouty, to whom even Dr Dundas allows the use of wine, we must regard this liquor as extremely hurtful; and the only safe article which can be used by persons with such complaints is the pure element.*

In the subsequent observations on the medical and hygienic employment of baths, Dr Dundas expresses the opinion, that practitioners will obtain important advantages from the modern system of hydropathy, judiciously modified. He is satisfied, he says, that some form of the hydropathic treatment will maintain a permanent place in scientific medicine, "after the mass of obscurity in which it is overwhelmed, through the ignorance and quackery of its present apostles, has been forgotten;" and he adds that, "in the wards of the

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* Since the above was written, indications of the apprehensions of the wine-trade, and of the determination of the Government not to touch these duties, have reappeared.

On the evening of Thursday, June 17, 1852, Mr Mullings, in the House of Commons, asked the Chancellor of the Exchequer, if he was prepared to give such an assurance as would allay the apprehension at present prevailing throughout the wine-trade, by the announcement of a project for reducing the import duty to one shilling per gallon,—a rumour which was calculated to raise the price of wine abroad, and was already operating to the injury of the revenue, by deterring parties from taking wine out of bond for home-consumption.

The Chancellor of the Exchequer answered, that there was no project on the part of Her Majesty's Government to reduce the import duty to one shilling per gallon; that he trusted that no Government that would exist in this country would ever entertain that project; that there was no intention on the part of Her Majesty's Government to recommend any reduction in the duties on wine; and that, independent of the probable loss to the revenue, there were many other claims far superior to those of the wine-duties for a reduction of taxation.

This statement was repeated by the Chancellor of the Exchequer, on the evening of Friday, the 18th June, in answer to a question, and some similar remarks made by Mr Anstey, the original mover of the committee for inquiry. If it was in the power of government, the Chancellor of the Exchequer repeated, to deal with a surplus revenue, so as to permit them to reduce taxation, the wine-duties were not among those imports which they were inclined to remit.

This is quite distinct; so that all those who recommend the use of French and German wines in preference to the spirituous compounds of Portugal, and all those who recommend even good Peninsular wines in preference to brandy and water, may cease to offer any suggestions or remonstrances, or may recommend the use of water alone.

Northern Hospital of Liverpool, he has demonstrated its great value in certain forms of chronic rheumatism.'' This is not saying a great deal for hydropathy. A judicious modification of it has long been in use, before the late reviver of the system brought it into indiscriminate and injudicious employment. To say that it is "overwhelmed in obscurity,'' is an expression which to us is unintelligible. There has been no obscurity about its applications; but there has been infinite rashness, total want of discrimination, and entire disregard to the states of the system, in which it was or was not applicable. In such circumstances, hydropathic treatment cannot be regarded as serviceable. It is certain that it has been often hurtful; and many of the instances in which it has not done mischief, may without injustice be regarded as escapes.

It is remarkable, amidst this commendation, of a method of undoubted efficacy, that an observer, so acute and original and so great a reformer as Dr Dundas, does not perceive, that by far the most serviceable and beneficial part of the cold water treatment is the internal use of that liquid as a drink. If good has been done by the practice, it is not so much by the use of cold sheets and cold-water washing as by the employment of cold water, instead of the wine and beer, and spirituous potations, which sent so many patients to the cold-water establishments. How many of these patients owed their ailments to the liberal use of wine, brandy and water, and similar liquids it is unnecessary here to say. The withdrawal of these poisonous stimulants alone was a great service. The substitution of cold water was also a beneficial proceeding, and with exercise and fresh air, they were quite sufficient to effect recoveries where the patient recovered, without either wet-sheet packing or any other means, except the cold shower-bath where it was admissible, and the tepid or warm bath, where it was not.

Dr Dundas pronounces a very decided condemnation on the black draught and blue pill of Mr Abernethy and his followers, and indeed upon the whole system of exhibiting purgatives and tonics. It is scarcely to be doubted that blue pills and calomel have been very much abused, and have been administered in a routine and thoughtless manner by practitioners who take no trouble in thinking on the cases of their patients. It is also a great evil that black draught has been and still is employed, where undoubtedly more gentle means, or no measures at all, might have answered the purpose. All this, however, is the effect of trusting to medicine and not to diet, both for the removal of uneasy sensations and the cure of diseases. Daily observation shews that the great majority of what are called the easy classes, eat about three or four times more food than what is either good or necessary. Then, when the effects of the daily surfeit begin to act upon the stomach, the circulation, and the secretions, instead of diminishing the amount of food taken, recourse is had to the blue pill and black draught; after which the patient dines again with great activity and enjoyment for a day or two; and is again ready for another dose. If, instead of all this feeding and medicine-taking, the patient were peremptorily obliged to eat about one-fourth of his previous allowance, all the uneasy symptoms would vanish; he would digest his moderate meal; and have some relish for the ensuing one, however humble.

In the third lecture, Dr Dundas enters upon the subject, which he manifestly considers as the great one of his book. the exposition of the erroneous views current on the nature and cause of Fever, and the demonstration of the new views on tropical and European fever, which it is his object to inculcate; and to this subject he devotes the third, fourth, fifth, sixth, and seventh lectures.

The author begins by reminding his auditors and readers that the doctrine that Marsh Miasmata, or what is called by the modern Italian term Malaria, is the cause, and the only cause, of intermittent fever, is believed to have been first promulgated in the year 1695, by Lancisi, the Roman physician; and that this doctrine has been adopted by almost universal consent, by teachers of medicine, and by practical men, as the sole and only cause of intermittent fever. He finds, however, that no one of the present day reads, or has read, the writings of Lancisi; and he infers that they are quoted by many who never read or saw them.—(P. 123.) He then finds that Lancisi cannot be deemed the author of the hypothesis of a special poison, such as that of the effluvia issuing from marshes.—(P. 127.) This, he thinks, must be considered as the doctrine of his commentators,—an inference which he deduces from the representations of Dr Thomas Watson.

Lancisi, he continues, propounds, as part of his doctrine, that two kinds of poisonous emanations are disengaged from waters in a state of corruption; one altogether inorganic in nature; the other distinctly organic in character. The former he conceives to be an accumulation of dead and inorganic particles with impure sulphur, acrid and volatile salts, with other extraneous matters, which, being densely evolved in the exhalations from the water, affect in an unpleasant manner the sense of smell. The other germs of effluvia are composed of a multitude of worms and *ova*, which float about in the atmosphere,—a distinct host of aerial animalcula.--(P. 131.)

Hence, Dr Dundas infers, it follows, that the constitution of the vegetable or marsh miasma, is altogether of a heterogeneous and contradictory nature, and when allowed to act on the human body, must produce very diverse effects. Whatever opinions the moderns ascribe to Lancisi, the perusal of his works sufficiently proves that he had no peculiar favourite theory to support.—(P. 147.) He allows that intermittent fever is generated independently of all vegetable or marsh effluvia.—(P. 148.) Perhaps some of his observations may be thought to prove too much. But they demonstrate clearly and expressly this fact, that Lancisi never entertained or promulgated the idea of a poison being the invariable cause of intermittent fever.—(P. 148.)

Dr Dundas then presents, as a condensed summary of the views entertained by Lancisi, on the subject of marsh poison and its effects, the following aphorisms.

1. That the poisonous emanations of marshes are not of a simple nature, but always compound.
2. That they are composed of inorganic and organic effluvia.
3. That the inorganic portion can scarcely be held to be of itself a cause of disease.
4. That the organic constituent, which is almost wholly of animal composition, conjointly with the preceding, occasions the fevers, &c.
5. That these emanations are consequently of a very compound character.
6. That these emanations or effluvia vary very much in their physical and chemical constitution.
7. That they are more especially influenced by the season of the year.
8. That they produce a diversity of diseases.
9. That they excite continued and remittent fevers as well as intermittent, and other disorders, as diarrhœa, &c.
10. That the fevers are not in all instances produced by marsh air or miasma; but that they occasionally arise from a disturbed state of the elasticity of the atmosphere.
11. That they are materially influenced by worms taken up by the animal body.
12. That fevers occasionally arise where the atmosphere is in the state of the highest purity.
13. Lastly, that Lancisi never employs throughout his work the word *venenum* or poison; (the author forgets the quotation which he gives at page 125;) and consequently he cannot be charged with the theory of marsh miasm being a

specific poison. The diversity of effects which he ascribes to its agency fully contradict any such proposition. Lancisi, in short, has been quoted, not read.—(P. 151–153.)

Those who are acquainted with the history of medicine and the doctrines taught, as well as the facts collected, on the nature and causes of Febrile Diseases, will probably not be able to recognise any very remarkable discoveries in the propositions now stated.

There is certainly nothing new in the information that Lancisi believed that intermittent fevers do not proceed from the emanations of marshy grounds alone; and that these emanations may cause not intermittent fevers but remittent and continued fevers. If Dr Dundas had extended his inquiries a little farther, he would have found that the predecessors and contemporaries of Lancisi all regarded Intermittent, Remittent, and Continued Fevers as they are seen in Italy, to be all pathologically and etiologically related; that the fevers which are called Continued co-existed in the same epidemic with the Intermittent and Remittent; that in some seasons these exhalations seemed to produce dysentery and cholera; in other seasons, febrile disorders which they called apoplexy and peripneumony; and in others disorders which they called Rheumatism. Lancisi himself describes an epidemic of apoplectic attacks which took place at Rome in the years 1705 and 1706, and which were manifestly the comatose tertians, and semitertians. Some, he allows, died in consequence of great and obstinate spasm or palsy of the *præcordia*, or obstruction of the outlets of the heart and large vessels. It was uniformly observed, he adds, that none of these persons who were thus suddenly cut off, did not labour under some disease of the fluids or of the vital organs. This was the predisposing cause, but the exciting cause was the *Intemperies* of the seasons.*

Again, Lancisi describes as taking place in 1709 at Rome, an epidemic of rheumatism, a long with the usual acute febrile disorders, and which appears to have been complicated with or accompanied by, inflammation of the throat, of the windpipe, the pleura and surface of the lungs, and occasionally all the organs of the chest.†

Again Lancisi describes various pestilences among cattle and horses; all of which he more or less ascribes to the same general cause of peculiar order or disorder of season and weather.

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* De Subitaneis Mortibus Libri Duo. Vide Librum Secundum; Caput Primum apud Opera Joannis Mariae Lancisii. Tomum I. p. 73. Romae, 1745.

† Epidemia Rheumatica Quae cum Acutis Febribus Romae pervagata est hyeme praesertim anni MDCCIX., &c., p 103, apud Dissertationem de Nativis et Adventitiis Romani Cœli Qualitatibus. Opera, tom. II.

It may or it may not be the case, that Lancisi does not promulgate the doctrine that marsh miasma are the cause of intermittent fevers. The inquiry into the authorship is not very profitable; and it is perhaps of less moment to know who first proposed the doctrine, than whether it is correct, and rests on good or uncertain foundation, how far it requires to be modified, and what are the admitted causes of fever among good observers at the present time. It may be the case that the doctrine has been modified by subsequent observers, and has required both to be modified and to be rectified.

It is neither necessary nor convenient at present to defend the doctrines of Lancisi, and to explain the merits and services of that learned and accurate observer. If any one wishes to understand the nature of these services, he will find it best to study with his own eyes the whole of the writings of that physician, if any one can be expected to do so in the present hurried and rapid mode of existence. To do so advantageously, he should be aware of the following circumstances.

Rome, and the Roman territory, and indeed almost the whole of the Peninsula of Italy, had, for many centuries, been subject to severe epidemical attacks of fevers, both intermittent, remittent, and continuous. Intermittent fever may be regarded as the constant endemic of the whole Peninsula. But during certain seasons, violent attacks of remittent and continued fevers prevailed in the summer and autumn in almost all the towns of Italy, especially towards the sea-coast. Such was the case with Rome, Terracina, Bagnarea, Venice, Modena.

Of all these places, however, Rome and the Roman territory suffered most severely. Almost every summer, febrile epidemics, of the most unmanageable and mortal character, appeared in the city and its suburbs, and were the means of cutting off great numbers of individuals, and sometimes entire families. One of these severe epidemics we mentioned in volume seventy-second as having taken place in the summer of 1649.* But they were of frequent recurrence, and had become so common and destructive in the sixteenth and the seventeenth centuries, that all the physicians, and many ecclesiastics of high rank, anxiously directed their attention to the probable causes of such frequent sickliness, and mortality so great.

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* Edinburgh Medical and Surgical Journal, vol. lxxii. p. 214, July 1849.

These persons could not be made to believe that Rome, the Eternal City, the seat of the Holy Father, the head of the church, was an unhealthy abode. They seemed to have imagined that a city, placed, as they believed, under the immediate care of the Supreme Being and the Holy Virgin, could not fail to be the most salubrious in the world. Any attempt to shew the true facts, that the causes of the sickliness of Rome were contained within herself, and in the habits of her inhabitants, would have been regarded as something profane and sacrilegious, and would have been rather dangerous for the person who attempted to do so.

Another reason, not less potential, obliged all the ecclesiastics at least, and probably others, to represent Rome as a very healthy city. The great confluence of strangers from all parts of Christendom, either for religious or secular purposes, could only be maintained by making it be believed that Rome was not only not dangerous to health, but highly salubrious. Had it been proved, as it appears often to have been reported, that the city of Rome, and the patrimony of St Peter, were not so free from diseases as was believed, the simple rumour might have diminished the revenues of the Pope and Cardinals, and all their dependents, to an extreme alarming to contemplate, and quite ruinous in its effects. Prove the city to be unhealthy, and the Pope and Cardinals were threatened with poverty and indigence greatly more apostolical than was either dignified or comfortable.

All the Roman physicians, accordingly, previous to the time of Lancisi, touch on this subject in the most delicate and transitory manner; and any hints which they give are of the most cautious description. The heat of the summer, the hot, windless state of the atmosphere in the autumn, a winter unusually cold, and a season of great drought or unwonted rains, inundations of the rivers, pits for macerating hemp, bodies of men and animals unburied, are several times noted as the cause of epidemic sickliness; and, when these failed, there was nothing to have recourse to, but the sins and iniquities of the people, and the wrath and displeasure of God the Father and the Blessed Virgin. As in the ancient city, a *Lectisternium*, or driving a nail into the wall, was the grand remedy for all evils—a defeat, a tempest, a famine, a pestilence; so, in the modern city, the principal remedy was a severe and strict fast, a mass of unusual solemnity, a procession of priests to St John Lateran, or a large addition to the number of psalms to be said or sung at complines and vespers.

At length Giovanni Maria Lancisi, physician to the Pope, in the end of the seventeenth century, directed his mind to the matter; but with great caution, and in the most prudent manner possible. Lancisi was probably secretly aware that Rome was not a very salubrious city; and this fact he thought ought to be made known as the primary foundation for any remedial measures, or as they would be called in the present day, sanatory proceedings. The way which he devised, was to distinguish the physical properties of the Roman climate and atmosphere into two orders:—the native or original, and the secondary or acquired. The native or original qualities of the Roman climate he maintained were excellent. They could not be better. The Roman soil was productive of the most valuable herbs, and was fertile in all articles. The complexion of the Roman citizens was florid and healthy. The Roman genius was unrivalled. No vernacular or home-born diseases were found prevalent among the citizens of Rome, who were further remarkable for longevity; and many persons who died after their seventieth year, departed rather from not changing their mode of life, than in consequence of the operation of the climacteric year. What this mode of life was, it is unnecessary to say. But by such arguments as these, Lancisi convinced himself, and probably the Pope and the Cardinals, that Rome was originally and ought to be a most healthy city.

So far does Lancisi carry this fancy of the innate healthiness of the Roman soil and Roman air, that he finds he cannot agree with Alexander Petronius, who had in no ambiguous language pronounced a sentence of decided insalubrity upon certain regions of the city, for no other reason, but because they were exposed to, and received the South winds. One of the places to which Petronius particularly refers, is the Valley of the Gardens, which extended from the Church of St Andrew or *delle Fratte*, and the base of the Pincian Hill, to the Flaminian Gate. This spot Lancisi maintains, though it might be unhealthy in former years, to be then completely delivered from this censure; because new buildings were erected, and the soil was dried. He allows, however, that one corner of the Pincian Hill, commonly called *Orto di Napoli*, where pot-herbs are reared, and a system of irrigation is carried on, from the moist muddy soil which it possesses in the summer, should be avoided by strangers in the autumn as a suspicious place.*

To all this representation of the salubrity of the city, any one might have justly replied, how does it happen that you have such frequent visitations of violent and unmanageable epidemics? How happens it, that every summer and autumn, almost, the sickness is so great, fevers so numerous, and deaths so frequent? With the greatest gravity Lancisi presents the ready reply. The native salubrity of this city is sometimes impaired; and from this source common and epidemic diseases proceed. He then proceeds to shew, that this occasional disturbance in the public health, arises from adventitious or acquired circumstances entirely. These he refers to two great heads; *first*, the hurtful effluvia arising from pools, marshes, and muddy places, aided by the fostering powers of the south wind; and, *secondly*, the ungenial north wind, and the extreme distemperature thence resulting, (*Intemperies,*) attacking suddenly and unusually, especially in the winter season, or lasting obstinately in spring. From these causes, he says, proceed fevers, especially camp fevers (in these days military operations were frequent); rheumatisms and acute fevers, with inflammations of the throat and chest.

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* De Nativis Romani Cœli Qualitatibus, cap. iv., p. 14; Opera Lancisii. Tome ii.

Lancisi takes a view of all the places in the city and suburbs of Rome, which suffer under these adventitious and acquired noxious qualities; and they are numerous and extensive. He is perfectly acquainted with the facts and opinions given by Livy, Dionysius of Halicarnassus, Dio Cassius, Strabo, and a host of modern writers, who had written on the inundations of the Tiber, and shewn the evil consequences of these inundations to the public health. They left lagoons (*Lacunæ*), pools, and marshes in certain places, muddy damp soil in others; and these, partly by the indolence and carelessness of the inhabitants, partly by the desertion of the lands, and the small number of persons who might keep the places clean and the grounds in cultivation, being acted on by the winds, especially the south, became the fertile source of all sorts of dangerous epidemic diseases.

These evils were not new or of recent origin. Their effects at all events had been observed between the twelfth and thirteenth centuries, in the time of Innocent the Third. They were observed in the fourteenth century, in the time of Clement the Fifth; and so great was the depopulation of the city from this and political causes, that when Gregory the Eleventh, in the year 1376, brought back the seat of the Triple Crown to Rome, he found the public and private buildings completely in ruins, and the inhabitants so reduced in number, that there were not more than 33,000 souls in the city.

The suburbs contained various villas and vineyards placed beyond the Coelimontan, the Latine, the Capuan, and Ostian gates, and on the banks of two small sluggish streams, tributaries of the Tiber, the Marian and Almon; and, from neglect, the grounds in these places were often wet, muddy, and in the state of small marshes. All these places, he had remarked, were more productive of disease, especially febrile diseases, after the period of the summer solstice, and during the whole of autumn more particularly, if either there were no winds to promote the safest mode of desiccation, or if the winds were from the south or the south-west (*Africus*).

Lancisi might be mistaken in his opinion as to the cause, and the supposed connection of the facts with the alleged cause. As to the facts themselves, he could not be mistaken; for these were manifest to the whole population of Rome.

To one cause perhaps Lancisi has not adverted so fully and so directly as he might have done. The ruins of houses, temples, and public buildings, which had been allowed in various places to accumulate during several centuries, had tended greatly to obstruct ventilation. In some instances the different drains or sewers leading into the *Cloaca Maxima* were obstructed. In many places rubbish had been accumulated to the elevation of from six to twelve and twenty feet above the site of the old streets. All these circumstances, and the impediments which they caused to perfect drainage and ventilation, tended to load the air of the city with bad and foreign vapours. When the rains of a wet season had fallen and filled these places with water, or when the Tiber had inundated the lower part of the city, filling the cellars and other places with water, it often happened, that they were allowed for years to remain without any attempt being made to draw off the water and dry the wetted surface. A mixture of moist mud was thus left from season to season, until almost the whole of the lower parts of Rome on both sides of the river were something of the nature of an extensive accumulation of black filthy mud.

It must not be inferred, from what is now said, that Lancisi overlooked this source of insalubrity. He expressly says that his Holiness, evidently by the suggestion of Lancisi, had given immediate orders in the year 1709, that not only the streets and cross ways, but all the drains or sewers which drained the lower parts of the city, and also the wine-cellars, which the overflowing Tiber had inundated, together with the ditches and the moats of Hadrian's Castle, and the adjoining meadows should be thoroughly cleansed from all mud and mire.

In this inquiry, in which it appears Lancisi had been engaged more or less from the year 1674, that is for thirty-five years, it is quite clear that his first object was to discover the causes of the frequently recurring and constant sickliness of the city, and not the cause of Intermittent Fever. Ague, indeed, or Intermittent Fever, made but a small proportion of the diseases incident to Rome, the Roman territory, and the cities and towns of Italy. By far the most common disease was what we would call Continued Fever, with violent, and often fatal determination to the brain, the lungs and heart, and, in less numerous instances, to the stomach and liver or intestines. This disease was seen occasionally as a Remittent or Semitertian; but the remissions were so imperfect and imperceptible that they were called either Continuous, Subcontinuous, or Subintrant Fevers. It was, in truth, the endemic fever of ancient Rome, noticed by Livy, Dionysius of Halicarnassus, and Strabo, witnessed and mentioned by Horace, as the summer and autumn fever of the city, and described and treated by Asclepiades and Galen, sometimes as the Semitertian or *Hemitritaeus*, sometimes as the *Amphimerina*, but always known as a most dangerous and destructive disease. It was to discover the cause of these diseases that Lancisi undertook the inquiry which we have now mentioned. It hence results that it is not altogether just to say, that he traces and ascribes Intermittent Fever to marsh *miasmata*. With Intermittent Fever, as we have said, he had little or nothing to do. The object was primarily and principally to discover the causes of the diseases so generally prevalent and so fatal at Rome, and to shew, if possible, that these causes were not inherent in the situation and climate of Rome, but were altogether acquired under the course of numerous years of adversity;—foreign invasion, destruction, and plunder, internal dissension, negligence, and profligacy.

He then directed his attention to the subject of the exhalations of marshes generally as causes of disease, and studied to extend his doctrine to other districts and regions of Italy.

The Dissertation on the Native and Adventitious Qualities of the Roman Climate is dated on the fifth day before the Kalends of October, or the 27th September 1710. The Inquiry on the Hurtful Effects of the Effluvia of Marshes did not receive the sacerdotal *imprimatur* until the sixth day before the Ides of October, or the 10th of October 1716. In the former Dissertation the author makes occasional references to the Inquiry *de Noxiis Paludum Effluviis;* but it is invariably in the future tense, as a subject on which, though he was then engaged, yet with which he had not been so long familiar as with the first, and as of a performance which was not yet completed. *Demonstrabimus; Ostendemus;* are the terms always employed.

It may here be noticed that Lancisi was not the first who had ascribed the sickliness of the city of Rome and its territory to the operation of the marshy and wet ground within and around the city. An idea of this kind as to the cause of fevers was common among the Italian physicians and architects. The agency of marshes, pools, wet grounds in general, inundations of rivers, rice fields, pits for macerating hemp, unburied bodies of men and animals, and similar sources of contamination of the atmosphere, are constantly mentioned by all the writers of the sixteenth and seventeenth centuries, as antecedents of pestilential fevers. These causes are repeatedly stated by Nicolaus Massa, Francisco Alphani, Antonius Portus, Baldassar Pisanelli, Hieronymo Donzellini, Prosper Alpinus, and others of the sixteenth century; and they are familiarly mentioned by a writer, who calls himself Troilo Lancetta, and who published at Venice, on the 16th of June 1632, a Short Treatise on Pestilence then prevailing at Venice, and on the means of arresting its progress.*

Bernardini Ramazzini, also a laborious and learned physician of Modena, had observed with great care, the constitutions and diseases of that city and the neighbouring district, during the years 1690, 1691, 1692, 1693, 1694. The four or five previous years, Ramazzini observes, had been remarkable for great drought. In the spring of 1689 great rains began to fall; and this change was followed by destructive mildew on the corn and legumes. The rains increased and continued till the year 1690, during which the weather was cold, the sky clouded and overcast, much rain fell, and the Po and other rivers overflowed their banks and laid the country under water. No remarkable diseases took place in the winter. But as soon as the weather became warm, intermitting fevers became numerous in the spring;

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* "Having prepared the bodies of men for defence; if the plague shall be simple, having investigated with certainty its origin; for instance marshes, caverns, common sewers, bodies of animals unburied, and other impurities of the water and of the earth, immediately the principal foundation of the so-called exhalation is removed; then against the impurity already introduced, fires are to be kindled in every part of the city, and chiefly where the beginning and origin of the pestilence appears, as also in the places deprived of the benefit of the Sun."—Di Pestilenza Commune a Bruti, et di Contaggio Mortale dell' Huomo. Opera di Troilo Lancetta Utile et Importanti a Principe per impedirne L'Origine nelli Affari di Guerra, &c. In Venetia Appresso li Guerigli MDCXXXII., Small Folio, cap. 14, p. 42.

It is very likely that Troilo Lancetta is an assumed name; as this was a custom not uncommon in those days.

and double tertians and semitertians, in fact remittents and subcontinuous fevers appeared in summer and autumn. With these, worms in the bodies of patients were numerous and frequent; the parotid glands often suppurated; suppurating diarrhœas were frequent. Animals also were sickly.

In 1691, apoplexies, quinsies, catarrhs, pleurisies, and inflammations of the lungs, were the prevalent diseases; all which Ramazzini ascribes to the unhealthy state of the air induced by the wet condition of the country.

All these phenomena Ramazzini had observed previously to Lancisi, or about the same time, certainly not after him. His works were published in 1717 at London; but the treatise now mentioned had previously appeared at Modena.*

Lancisi, however, had the merit of collecting all the previous facts and information, and making them illustrate the etiology of epidemic and endemic diseases in general; and he urged upon the Papal Government more forcibly and decidedly than heretofore, the necessity of adopting sanatory measures.

Other observers, however, in a different field, were collecting facts and observations on the same subject.

The Austrian empire and the kingdom of Hungary had long been engaged in warfare with Turkey. The Austrian and Hungarian troops were almost constantly in the field in Hungary. There they became acquainted with several most violent and unmanageable diseases of febrile character; the Hungarian sickness or Death-Fever, the Hagymaz or Remittent, Dysentery, Diarrhœa, Cholera, and Scurvy. The death-fever and the Hagymaz, with the Tsomoer or *Languor Pannonicus*, were most prevalent in the summer and autumn, and destroyed immense numbers of the troops. The physicians attached to the Austrian and Hungarian forces, regarded all these diseases as the offspring of the moist surface of Hungary, watered as it is along its southern frontier by the slowly rolling Danube, and intersected by the Drave, the Save, the Teisse, and a multitudinous body of tributaries, which traverse the country like an immense aqueous network. Certain it is that during the summer season when the solar heat becomes intense, between the superficial desiccation and evaporation during the day, and the dense dews and fogs during the night, all the diseases depending on the surface of the soil and the state of the atmosphere, prevail there with a degree of force and virulence, as well as rapidity of progress, little known in other parts of Europe, excepting Spain and the peninsula of Italy.

All this had been known to the physicians and surgeons of the Austrian armies, when in 1684 particular attention was directed to the subject by an Italian physician. Luca-Antonio Porzio, a native of Naples, and professor of medicine and anatomy in the Royal University of Naples, was induced, in consequence of great sickliness at Vienna in 1684, to perform a medical tour from Venice by Vicenza, Verona, Trent, Innspruck, Moenichen, and Linz, to the capital of the Austrian empire. There he witnessed not only the domestic and native diseases of Vienna, but the diseases presented by the soldiers who had returned thither in bad health, from the encampments at Buda and at Gran, where a large force had been assembled. Hundreds had already died of fever, dysentery, diarrhœa, and cholera. Those who returned were afflicted by dysentery and diarrhœa; tertian and quartan agues and other fevers of an uncertain type, evidently the relics of the Pannonian sickness; visceral enlargements and swellings of the abdomen, with œdematous swellings of the feet, and symptoms indicating disease of the lungs. Porzio published at Vienna in 1685, as the result of his observations, a Treatise *De Militis Sanitate tuenda*, a sensible work on military medicine and hygiene, in which all the facts stated, and the principles inculcated, were derived from the experience of physicians in the Hungarian campaigns.*

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* Bernardini Ramazzini Opera omnia Medica et Physica. London, 1717.

Luca-Antonio Porzio shews in this Treatise, that he was quite familiar with the effects of marshy wet ground, and encampments on a wet well-watered country, rather low, like Hungary, upon the health of large bodies of human beings. Though he does not overlook the effects of intemperance in causing disease, yet he surveys with a skilful eye the hydraulic system of the Danube and his native Adige, and contrasts the circumstances of a slowly flowing body of water like the Danube and its tributaries, with the rapid descent of a mountain stream like the Adige, in order to explain the different effects of the influence of the banks of these rivers upon the health of the inhabitants. The long course of the Danube, and the sluggish streams which supply its waters, the bad character of the country through which they flow, he regards as the main causes of the insalubrity of Hungary.

Whether Porzio proves that agues and remittent fevers arise from marshy ground or not, it must be allowed that he adduces good evidence to shew, that an alluvial country with a wet surface is in the summer and autumn a most unhealthy country, and productive of many dangerous disorders.

Contemporary with Ramazzini was Francis Torti of Modena. His work entitled Therapeutice Specialis, contains a faithful account of the Italian fevers, as seen at Modena, especially the pernicious Subintrant Fevers. But his object was to distinguish and cure these disorders; and of their causes he says little or nothing.

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* Lucae Antonii Portii Opera omnia Medica Philosophica et Mathematica, curâ Francisci Portii. 2 vols. 4to. Neapoli, 1736.

The same character is applicable to the work of Abraham Werlhof, which appeared in 1745.

In 1751, appeared the Classical Treatise of George Cleghorn on the Epidemic Diseases prevalent in the Island of Minorca; and in 1752, the not less instructive work of Sir John Pringle on the Diseases of the Army, especially in the Low Countries. The first writer was anxious to give a correcta ccount of the Tertian fever, and especially the Semitertians of Minorca: and though he thinks these connected with marshy and imperfectly-drained ground, he connects them with the season, the atmospheric temperature, and the great dryness produced after long-continued and rather intense heat.

The doctrines of Sir John Pringle are too well known to require particular notice.

In 1768, Dr James Lind of Haslar Hospital, Portsmouth, published his Essay on Diseases Incidental to Europeans in Hot Climates;[*] and in 1772, his namesake and relative Dr James Lind gave to the world his Treatise on the Putrid and Remitting Fen Fever, which raged at Bengal in the year 1762.[†]

The former Treatise is remarkable for the extensive information which the author had collected on every important spot almost on the globe, its characteristic diseases, and the physical causes to which they seemed most likely to be owing. He is a faithful and correct observer of facts, and avoids giving opinions and speculations. The facts which he has collected, without being exactly intended to do so, form probably the largest amount of evidence hitherto adduced, to shew the hurtful nature of low alluvial countries, sea-coasts, low outlets, and banks of rivers. Two leading inferences result from his observations;—namely, that the land and the sea-coast in tropical and warm countries is productive of fever and similar diseases; and that the sea and the sea-air is most salutary and preventive, even curative, of disease.

The Essay of his relative and namesake contains an excellent account of the physical characters of the river banks of the Ganges, and a correct description of the East India fever, both of the low humid districts, and of the jungles and woods.

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[*] An Essay on Diseases Incidental to Europeans in Hot Climates. With the Method of preventing their Fatal Consequences. By James Lind, Physician to Her Majesty's Royal Hospital at Haslar, near Portsmouth, &c. London, 1768. 8vo.

[†] A Treatise on the Putrid and Remittent Fen Fever, &c. By James Lind, M.D., &c. London, 1772. 12mo.

Dr James Lind junior, allows, however, that, next to fenny miasmata, cold is to be reckoned as the strongest exciting cause of the Bengal and Gangetic fever; and, he adds, many respectable physicians are of opinion that cold alone, provided the body is sufficiently predisposed, is sufficient to generate a disease perfectly like that which is produced by the Fenny *Miasmata.*[*]

It will subsequently appear, that, by this small speculation, Dr James Lind, junior, has anticipated the hypothesis proposed by Dr Dundas.

Dr Dundas, it may here be observed, has performed a remarkable feat. He has created a new author, and invested him with the honour of knighthood. The Sir John Lind whom he several times mentions is not known in the history of medicine, or the records of eudemial diseases. The only persons bearing the name of Lind, are the two physicians now mentioned, both James Lind, both physicians, and fellows of the Edinburgh College; but neither dignified by any other title.

Numerous authors published the result of their observations on fevers, and their causes in different parts of the globe. It was upon the whole agreed that wet and marshy situations were productive of various diseases; Fever, especially the Remittent and Subcontinuous, Yellow Fever, Dysentery, Diarrhœa, Cholera, and occasionally other distempers. But it was further allowed, that a peculiar train of weather and season was also requisite. In 1772 there appeared at Antwerp, and various other towns of Belgium, and other European countries, a Continuous Remittent fever, with bilioso-putrid symptoms,—in short, a semitertian and subintrant, with verminose complication. Of this epidemic a full description was given in 1774 by Peter Van Elsaker, a practical physician in Antwerp, preceded by a correct and instructive description of the physical and meteorological peculiarities of that ancient emporium.

The author here shews the influence of the site of Antwerp upon the right bank of the Scheldt, in rendering it liable to various diseases. But he is most full upon the weather and climate, the antecedents, which he thinks mainly contributed to the production of this epidemic. He gives also some interesting information upon the habits and modes of living of the inhabitants of Antwerp at that time.[†] Peter Van

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[*] A Treatise on the Putrid and Remitting Fen Fever, &c., p. 31.

[†] Petri Van Elsaker apud Antwerpiens. Medicin-Licentiat. Practic.

Elsaker believes in the hurtful influence of moist, low, marshy situations, towns especially intersected by canals, like Antwerp, Ghent, and Rotterdam. But he also attaches no small importance to the influence of the seasons and the weather; rain, solar heat, excessive cold in the winter, and the want of good ventilating winds.

Our limits scarcely allow us to follow the progress and vicissitudes of medical doctrine on this point at present. It is sufficient to say that by such writers, as Dr John Hunter in his Treatise on the Diseases of Jamaica, Hume, Moseley, Jackson, Bancroft, Irvine, Fergusson, Brown, Ranken, and many others, it was more or less clearly proved, that on the one hand marshy and wet low situations were peculiarly unhealthy and productive of fever; that on the other, what is commonly called a marsh, that is an open marsh, is not essential to the production of fever; and that the worst fevers may rise where no marshes are perceptible.

For instance, it has always been known that in Spain and Portugal, many places apparently dry and free from marsh, are highly productive of fever, when human beings are brought to dwell upon these places. In these countries, tertians, semitertians, and pernicious subintrant fevers prevail, where no visible marsh is seen. But it has been often found that a few inches beneath the surface, the soil is moist and spongy, indeed retains much water, and this seems to supply to the superficial soil acted on by the hot sun during summer and autumn, some peculiar matter, which inhaled into the human body, becomes the cause of bad fevers.

In 1810 Dr William Irvine published Some Observations upon Diseases chiefly as they occur in Sicily; a small but remarkable production; remarkable for the original information which it contains, both as to the causes and the treatment of fever. This book is almost the first in which the word *Malaria* occurs as a substitute for Miasmata. This term, Dr Irvine had learned in his intercourse in Sicily with the natives. The existence of Malaria he allows; its operation he does not deny; but he finds other causes concerned in the production of fevers, not so easily demonstrated. Solar heat in the summer, and a peculiar state of the atmosphere in autumn, he regards as potential causes of fever. Places called *Fiumari* in the language of the country, that is the water-courses or beds of mountain streams, which though filled with torrents in winter, become dry in summer, are extremely unhealthy, and are great sources of fever in summer

Specimen Medico-Practicum Febrem Remittentem Continuam Bilioso-Putridam, anno 1772, Antwerpiæ et per plures Belgii ex Europæ civitates epidemico impetu grassatam exhibens. Antwerpiæ, 1774.

and autumn. Sometimes these are ravines. In some instances, the water of these Fiumari sink into the ground and disappear for a time and space altogether. In such places, fever is very likely to be caught. Thus Sicily, though not a low flat country like Holland and Belgium, and the Italian coast, nay a mountainous country, is highly productive of fever.*

What is true of Sicily is true of all the Mediterranean islands;—Sardinia, Corfu, Cephalonia, Zante, and the islands of the Archipelago. All are productive, in different degrees, of fever.

The facts now referred to had long led to the inference, that the origin of endemic, and perhaps epidemic fevers is connected with the surface of the ground, and the atmosphere in some way not easily understood; and the origin was called simply Telluric and Atmospheric, without attempting to specify more particularly the exact nature of the emanation. For more particular accounts, we refer to the work mentioned at the foot of the page.†

Our limits permit us not to consider, or even mention, all the writers by whose researches the hypothesis of the influence of marsh miasmata has been assailed, modified, circumscribed, or attempted to be subverted. All have at different times received consideration, more or less full in this Journal, from Humboldt and Dr John Wilson,‡ down to the more recent attempts by Dr James Gayley,§ and Dr Mitchell of Philadelphia.‖ It would be wrong not to allow that the explanations given by Humboldt, Dr John Wilson, and Dr Mitchell, are entitled to infinitely more attention than those of the other opponents of this doctrine. To the present author, it is clear that all of them are completely unknown. It has been repeatedly proved, that the existence of marsh is not essential to the production of fever, and that something different from the marsh and its exhalations must be concerned in the production of the Remittent and Subcontinuous Fevers of hot, unhealthy seasons;—Breakbone Fever, Lake Fever, and Yellow Fever. This, nevertheless, Dr Dundas tries to prove once more; and the manner in which this is done, must be new pretty well understood.

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* Some Observations upon Diseases, chiefly as they occur in Sicily. By William Irvine. M.D., F.R.S.E., of the R. C. of Physicians of London, and Physician to His Majesty's Forces. London, 1810. 8vo.

† Elements of Practice of Medicine. By David Craigie, M.D., &c. Vol. i. Edinburgh, 1837. Book i., Chap. i. p. 71, &c., Chap. ii. p. 168, and Chap. iii. p. 212, &c.

‡ Memoirs of West Indian Fever. By John Wilson, M.D. London, 1827; and Edinburgh Medical and Surgical Journal, vol. xxix., p. 189. Edinburgh, 1828.

§ On the Etiology of Intermittent and Remittent Fever. By James F. Gayley. Edinburgh Medical and Surgical Journal, vol. lxxiii., p. 488.

‖ On the Cryptogamous Origin of Malarious and Epidemic Fevers. By J. K. Mitchell, A.M., M.D., Professor of Practical Medicine in Jefferson Medical College. Philadelphia, 1849. Small 8vo. Edinburgh Medical and Surgical Journal, vol. lxxiii., p. 477.

The agent or cause, to which Dr Dundas ascribes the production of all Fevers, Intermittent, Remittent, Continued and Typhus, is sea-air. A strong current of cool sea-air applied to the body when it is warm, enfeebled, or otherwise disordered, is sufficient, according to the experience of Dr Dundas, to produce all the phenomena, all the evils of Fever, whether Intermittent or Continued, Typhus or Typhoid.—(P. 232.) Marsh and marsh air are nothing; they are innocuous; probably beneficial. It is not to be denied that cold air and cold winds are the causes of much evil, and not a few attacks of disease; and what Dr Dundas states at page 247, of the chilling effect of the Tramontane winds in the streets and squares of Rome is perfectly credible. Neither is it to be denied that woollen clothing is of great value in protecting against attacks of disease, as well in hot and tropical countries, as in cold regions. But whether mere cool, sea-air, which Dr James Lind, senior, and many others consider perfectly harmless, and almost always salubrious, except in the case of a tempest, is a point which may be left to the decision of those, who know what it is to reside in the marshes of Sologne, in France, or in the city of New Orleans on the Mississippi, and to live in a seventy-four sailing up and down the Mediterranean.

Not that ships, and their interior, are in all instances safe. But they seem to be safer than the coast of Sierra Leone and the Bight of Benin.

To every one's recollection and knowledge must occur many facts which are at variance with the hypothesis of Dr Dundas. To say that the hypothesis proposed by Dr Dundas is not new, is no objection. It ought perhaps to be a recommendation, that the cause which he assigns was allowed to have a considerable degree of weight, by Dr James Lind, junior, and many other observers. But this concession scarcely touches the point at issue; and whoever peruses the writings of Humboldt, Dr Jackson, Dr Wilson, and Dr Mitchell, will see that the exact deprehension and revelation of the causes of fever is yet to be made.

Here, however, for the present, we pause; not without recommending to the consideration of readers, both medical and general, the perusal of the present work, as one containing much interesting information, and many things which it is important and desirable to know.

PART III.

MEDICAL INTELLIGENCE.

I.—PHARMACY.

On Socotrine Aloe Juice, or Liquid Socotrine Aloes. By JONATHAN PEREIRA, M.D., F.R.S., &c. (Pharmaceutical Journal and Transactions, April 1852, p. 439.)

IT has long been known that the Socotrine aloes imported into England varies considerably in its consistency, and is sometimes met with in a soft or semifluid state. Frequently, on opening a package of this sort of aloes, the interior is found to be quite soft, while the exterior is firm and hard. In general this arises from insufficient evaporation of the aloe juice.

In the third edition of the *Elements of Materia Medica* (vol. ii., part i., p. 1077), published in 1850, Dr Pereira briefly referred to a soft or semiliquid Socotrine aloes, which had a bright or palm-oil yellow colour, and a very fragrant odour. At that time he had little opportunity of investigating this drug; but a large importation of it having recently taken place, Dr Pereira has more fully examined it, and as it appeared to be the raw or unboiled juice of the plant yielding what is known in commerce as Socotrine aloes, he proposed to distinguish it from the ordinary soft Socotrine aloe by the name of "*Socotrine Aloe Juice.*"

Messrs Horner, the holders of the whole of the present importation of this juice, informed Dr Pereira that it was purchased of the Arabs up the Red Sea, by a merchant, who was assured by the vendors that it was very fine aloe juice, and had not been boiled or otherwise altered. It was imported into London by way of Madras, in casks, each containing six cwt. Dr Pereira is informed that the contents of some of the packages have undergone decomposition during the voyage.

Its consistence is that of treacle, or very thin honey; its colour, deep orange, or palm-oil yellow; its odour, powerful, fragrant, and resembling that of fine Socotrine aloes. By standing it separates into two parts,—an inferior, paler coloured, opaque, finely granular portion,—and a superior, darker coloured, transparent liquid. The latter forms, however, a very small portion of the whole mass.

When the granular portion is submitted to microscopic examination, it is found that the opacity and granular appearance arise from myriads of beautiful prismatic crystals. If a temperature of 132° Fahr. be applied to the juice, these crystals melt or dissolve, and the juice becomes deep red and transparent; and when the liquid becomes cold, it retains its transparency, and does not deposit any crystals. By evaporation the juice yields a solid, transparent extract, having all the characters of fine Socotrine aloes, in which no trace of crystalline texture can be discovered. Mr Jacob Bell has ascertained that 14 lb. of the juice yield 8 lb. 12 oz. of solid extract, or 62½ per cent. When the juice is mixed with cold distilled water, it becomes opaque yellow, and renders the water turbid, but is not miscible with it. If, however, heat be applied, the juice dissolves in the water, forming an almost clear, rich, red liquid. As the solution cools, it at first becomes turbid, owing to the separation of an opaque yellow precipitate, which apparently is the crystalline principle in an amorphous form. This gradually separates from the liquid, and collects as a clear resiniform mass (commonly called the *resin* of aloes) at the bottom of the vessel, leaving the supernatant liquid tolerably clear. If the juice be shaken up with rectified spirit of wine, a uniform clear mixture is obtained, from which numerous yellow crystals rapidly fall to the bottom of the liquid. Similar results are obtained when we mix the juice with equal parts of rectified spirit of wine and water.

This crystalline constituent of Socotrine aloes is, doubtless, either the *aloine* described by Messrs T. and H. Smith of Edinburgh, and by Dr Stenhouse, or a principle closely allied to it. Dr Stenhouse, to whom Dr Pereira gave a sample of it, is now engaged in its investigation; and in a letter which Dr P. received from him, he says, that though he has not been able to get the aloine ready for analysis, yet, from the experiments he has already made with it, he has scarcely a doubt that it will be found identical with that formerly obtained from Barbadoes aloes. It forms, he adds, a precisely similar combination with bromine, and, in short, agrees with it in every particular; Dr Pereira, therefore, provisionally terms this crystalline principle the *aloine of Socotrine aloes*. On comparing it with a fine specimen of aloine kindly presented to me by Messrs Smith, he finds its crystals smaller and more tapering, the summits of the crystals being more acute.

In drying, the crystals of the Socotrine aloine have a strong tendency to break up, so that crystals which, in the moist state, are moderately large and regular, become small and pulverulent when dry. Like the aloine crystals of Mr Smith, the aloine crystals of Socotrine aloes strongly doubly refract and depolarise light, and are therefore beautiful objects, when viewed by the polarising microscope.

The crystals of aloine contained in Socotrine aloe juice cannot be confounded with the crystals of oxalate and phosphate of lime found in the juices of various plants, and which are called by botanists *raphides*. The appearance under the microscope of the former is very different from that of the latter. Moreover, the ready fusibility, solubility, and complete combustibility of aloine crystals, easily distinguish them from the calcareous salts just referred to. On platinum foil, the aloine burns without leaving any residue, except such as may arise from the presence of traces of some foreign matter.

Aloine may be readily obtained from the juice by mixing the latter with spirit (either rectified or proof), and collecting and drying the precipitate. When procured in this way, it appears to the naked eye like a yellow powder, but, when examined by the microscope, it is found to consist of minute fragments of crystals.

The tincture from which the aloine has been separated yields by distillation a spirit having the fragrant odour of the juice; shewing that the latter contains some volatile odorous principle. By evaporation the tincture yields a resiniform extract.

In the first edition of his "Elements of Materia Medica," published in 1840, Dr Pereira stated, that by digesting hepatic aloes in rectified spirit of wine, a yellowish granular powder is obtained, which is insoluble in (cold) water, alcohol, ether, and dilute sulphuric acid, but is readily soluble in a solution of caustic potash, forming a red-coloured liquid. The powder-like residue, here referred to, is identical with the aloin of Socotrine aloes. When examined by the microscope, it is perceived to consist of very minute prismatic crystals, which depolarise polarised light like the larger crystals of aloin above referred to. He thinks, therefore, that it may be safely inferred, that hepatic aloes has been prepared without the employment of artificial heat, and that its opacity is due to the presence of minute crystals of aloine.

When Socotrine aloes is digested in rectified spirit, an insoluble portion is also obtained; but its colour, instead of being yellow, as in hepatic aloes, is dark-brown. On submitting this dark-brown insoluble portion to microscopic examination, he finds that it contains depolarising crystals.

Artificial Socotrine aloes (prepared by evaporating this aloe juice) also yields, when digested in rectified spirit, a dark-brown insoluble portion.

Dr Pereira, therefore, infers that Socotrine aloes differs from hepatic aloes in the circumstance of its having been prepared by the aid of artificial heat, by which its aloine constituent has become altered. This inference is further substantiated by the fact, that after it has been melted, hepatic aloes is found to have acquired the clearness and transparency of the Socotrine sort.

The clear supernatant portion of aloe juice, from which the above crystals have subsided, would probably also yield, by spontaneous evaporation, an extract resembling, or identical with, Socotrine aloes.

When the aloine was allowed to remain in the water-bath for more than six hours, it continued slowly to loose weight, apparently owing to its undergoing partial decomposition by the formation of a brownish resin. The loss of weight gradually continued for a week or more, but became very rapid when the aloine was heated to 302° F., when it melted, forming a dark brownish mass, which when cooled became as hard and brittle as colophonium. It still, however, contained a good deal of unaltered aloine, as was ascertained by crystallising it out with hot spirits and analysing it. Much of the aloine, however, had been changed, most probably by oxidation, into a dark brown uncrystallisable resin.

Brom-aloine.—When an excess of bromine is poured into a cold aqueous solution of aloine, a bright yellow precipitate is immediately produced, the amount of which increases on standing, while at the same time the supernatant liquid becomes very acid from containing free hydrobromic acid. The precipitate, after it has been washed with cold water to remove adhering acid, is dissolved in hot spirits of wine; and on the cooling of the solution it is deposited in bright yellow needles radiating from centres, which attach themselves to the bottom and sides of the containing vessel.

The crystals of brom-aloine are considerably broader than those of aloine, and have a richer yellow colour and a higher lustre. Brom-aloine is quite neutral to test-paper, is not so soluble in either cold water or cold spirits of wine as aloine, but dissolves very readily in hot spirits of wine.

Grm.
0·421 substance dried *in vacuo* gave 0·547 carbonic acid and 0·103 water.
0.856 gave 0·848 bromide of silver=42·16 Br.
0.300 substance gave 0·391 carbonic acid and 0·078 water.
0·661 substance gave 0·649 brom. of silver=0·2762 Br.=41·78 per cent.

	Calculated numbers.		I.	II.
34 C	2550·00	35·73	35·43	35·53
15 H	187·50	2·62	2·71	2.86
14 O	1400·00	19·63	19·70	19·83
3 Br	2998·89	42·02	42·16	41·78
	7136·39	100·00	100·00	100·00

The brom-aloine employed in these analyses was prepared at two different times. It is plain therefore, from these results, that this bromine compound is aloine, $C_{31} H_{18} O_{14}$, in which 3 equivs. of hydrogen are replaced by 3 equivs. of bromine. The formula of brom-aloine therefore is $C_{43} H_{15} O_{14} Br_3$.

When a stream of chlorine gas was sent for a considerable time through a cold aqueous solution of aloine, a deep yellow precipitate was produced. It contained a great deal of combined chlorine; but as it could not be made to crystallise, it was not subjected to analysis. In the present instance, and in those of several other feeble organic principles, such as orcine, chlorine appears to act somewhat too strongly, so that the constitution of the substance is destroyed, and merely uncrystallisable resins are produced. Bromine, on the other hand, is much more gentle in its operations, and usually simply replaces a moderate amount of the hydrogen in the substance, so that, as in the case of orcine and aloine, crystalline compounds are produced.

It has long been known to medical practitioners, that the aqueous extract of aloes is by far the most active preparation of that drug. The reason of this is now very plain, as the concentrated extract of aloes obtained by exhausting aloes with cold water consists chiefly of aloine, by much the larger portion of the resin being left undissolved. Mr Smith states that, from a series of pretty extensive trials, from 2 to 4 grs. of aloine have been found more effective than from 10 to 15 grs. of ordinary aloes. Aloine is, Dr Stenhouse thinks, therefore likely ere long to supersede, at least to a considerable extent, the administration of crude aloes.

Dr Stenhouse endeavoured to obtain aloine by operating considerable quantities of Barbadoes, Cape, and Socotrine aloes. These were macerated in cold water, and the aqueous solutions obtained were concentrated to the state of thin extracts on the water-bath. Dr Stenhouse was quite unsuccessful in every instance. The impurities contained in the extracts in these different kinds of aloes appear, when in contact with the oxygen of the air, to act upon the aloine so as effectually to prevent it from crystallising. Aloine can only therefore be obtained in a crystalline state by concentrating the cold aqueous solution of aloes *in vacuo;* though, after the aloine has once been crystallised, and it is freed from the presence of those impurities which appear to act so injuriously upon it, the aloine may be quite readily crystallised out of its aqueous solutions in the open air.

Though aloine has as yet only been obtained from Barbadoes aloes, Dr Stenhouse has no doubt that it also exists both in Cape and Socotrine aloes. The amount of aloine in Cape aloes is, however, in all probability, much smaller than in either of the other two species; for Cape aloes is well known to be a much feebler cathartic, and to contain a mass of impurities. In corroboration of this opinion, Dr Stenhouse referred to the fact already mentioned in a previous part of this paper, viz., that when aloine is digested with nitric acid, it is converted into Dr Schanck's chrysammic acid. Now it has been satisfactorily ascertained that all the three species of aloes yield chrysammic acid, of which in fact they are the only known sources. Cape aloes, as might have been expected, yields by far the smallest amount of chrysammic acid together with much oxalic and some nitropicric acids. There appears, therefore, great reason to believe that all the three kinds of aloes contain aloine.

Since the above was written, Dr Stenhouse learned from Mr Smith that he has not succeeded in obtaining crystallised aloine from either Cape or Socotrine aloes. Mr Smith does not doubt that both of these species of aloes also contain aloine, though, most probably contaminated with so much resin, or some other substances, as prevents it from crystallising. What tends to confirm Mr Smith in this opinion is the observation he has made, that when the crude crystals of aloine are allowed to remain in contact with the mother-liquor of the Barbadoes aloes, they disappear and become uncrystallisable. Dr Stenhouse also observed a similar occurrence in the mother-liquors of tolerably pure aloine. These become always darker and darker; so that if we continue to dissolve new quantities of aloine in them, at length scarcely any of it crystallises out, and the whole becomes changed into a dark-coloured magma.

In the year 1846, M. E. Robiquet published an account of an examination he had made of Socotrine aloes. By treating the concentrated aqueous solution of this species of aloes with basic acetate of lead, he obtained a brownish-yellow precipitate, which was collected on a filter, and washed with hot water. On decomposing this lead compound with sulphuretted hydrogen and evaporating the solution to dryness, he obtained an almost colourless varnish, consisting of a scaly mass, which was not in the least degree crystalline. M. Robiquet subjected this substance, which he called aloetine, to analysis, and obtained the following result:—

$$\begin{array}{rl} 8\,C = & 27{\cdot}7 \text{ per cent.} \\ 14\,H = & 10{\cdot}8 \quad \ldots \\ 10\,O = & 61{\cdot}5 \quad \ldots \\ \hline & 100{\cdot}0 \end{array}$$

It is plain therefore that M. E. Robiquet's aloetine, if it really is a definite organic principle, which Dr Stenhouse very much questions, is certainly a very different substance from the aloine which has formed the subject of the present notice.

That Socotrine and hepatic aloes were obtained from the same plant, and were not different species of aloes, Dr Pereira long suspected; and in the first edition of his work on Materia Medica, published in 1840, he observed that "the similarity of the odour of Socotrine and hepatic aloes leads to the suspicion that they are obtained from the same plant; and which is further confirmed by the two being sometimes brought over intermixed, the Socotrine occasionally forming a vein in a cask of the hepatic aloes."

This intermixture of the two sorts of aloes in the same cask might be explained by supposing that the consolidation of the clear portion of the juice has produced the so-called Socotrine aloes; while the opaque aloine containing portion of juice has yielded what is termed hepatic aloes.

In the third edition of Dr Pereira's work above alluded to, he states that the name of *opaque liver-coloured Socotrine aloes* might with propriety be applied to hepatic aloes. But until the present time he has been unable to offer a plausible explanation of the cause of the difference in these two commercial kinds of aloes.

From the preceding remarks he draws the following inferences:—

1. That *aloine* pre-exists in a crystalline form in the juice of Socotrine aloes.

2. That the substance which deposits as a decoction of Socotrine aloes cools, and which is usually termed the *resin* or the *resinoid* of Socotrine aloes, is the aloine in a modified state.

3. That hepatic aloes* is the juice of the Socotrine aloes plant which has been solidified without the aid of artificial heat.

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* By the term "*hepatic aloes*" is meant the opaque liver-coloured aloes imported into England from the East Indies (usually from Bombay). This sort of aloes is very different from the *hepatic Barbadoes aloes*, which formerly appears to have been exclusively called "hepatic aloes."

4. That hepatic aloes owes its opacity to the presence of minute crystals of aloine.

5. That the juice of Socotrine aloes yields, when evaporated by artificial heat, an extract possessing all the properties of commercial Socotrine aloes.

On Aloine, the Crystalline Cathartic Principle of Barbadoes Aloes. By JOHN STENHOUSE, LL.D., (London and Edinburgh Philosophical Magazine. London: March 1852.)

About two months previous to March 1852, Dr Stenhouse received from Mr Thomas Smith, apothecary, Edinburgh, a quantity of a brownish-yellow crystalline substance which he had obtained from Barbadoes aloes. Mr Smith's process consisted in pounding the previously dried aloes with a quantity of sand, so as to prevent its agglutinating, macerating the mass repeatedly with cold water, and then concentrating the liquors *in vacuo* to the consistence of a syrup. On remaining at rest in a cool place for two or three days, the concentrated extract became filled with a mass of small granular crystals of a brownish-yellow colour. This is the crude substance to which Mr Smith has given the name of Aloine, and which appears to constitute the cathartic principle of aloes. The brownish-yellow crystals obtained in this way are contaminated with a greenish-brown substance, which changes to brownish-black on exposure to the air, and still more rapidly when it is boiled. In order to purify the crystals of aloine, therefore, they must first be dried by pressure between folds of blotting-paper, and then repeatedly crystallised out of hot water till they have only a pale sulphur-yellow colour. The aqueous solutions of aloine must on no account be boiled, but simply heated to about 150° F., as at 212° F. aloine is rapidly oxidised and decomposed. By dissolving the purified crystals of aloine in hot spirits of wine, they are deposited, on the cooling of the solution, in small prismatic needles arranged in stars. When these crystals have a pale yellow colour, which does not change when they are dried in the air, they may be regarded as pure aloine.

Aloine is quite neutral to test-paper. Its taste is at first sweetish, but soon becomes intensely bitter. Aloine is not very soluble either in cold water or in cold spirits of wine; but if the water or the spirits of wine are even slightly warmed, the solubility of the aloine is exceedingly increased; the colour of these solutions is pale yellow. Aloine is also very readily dissolved by the carbonated and caustic fixed alkalies in the cold, forming a deep orange-yellow solution, which rapidly grows darker, owing to the oxidation which ensues. The effects of ammonia and its carbonate are precisely similar. When aloine is boiled either with alkalies or strong acids, it is rapidly changed into dark brown resins. A solution of bleaching-powder likewise gives aloine a deep orange colour, which soon changes to dark brown. Aloine produces no precipitate in solutions either of corrosive sublimate, nitrate of silver, or neutral acetate of lead, It also yields no precipitate with a dilute solution of subacetate of lead. but in a concentrated solution it throws down a deep yellow precipitate, which is pretty soluble in cold water, and is therefore difficult to wash. This precipitate is by no means very stable; and when it is exposed even for a short time to the air, it becomes brown.

When powdered aloine is thrown, in small quantities at a time, into cold fuming nitric acid, it dissolves without envolving any nitrous fumes, and forms a brownish-red solution. On adding a large quantity of sulphuric acid, a yellow precipitate falls, which, when it is washed with water to remove all adhering acid and then dried, explodes when it is heated. It plainly, therefore, contains combined nitric acid. I could not, however, succeed in obtaining this compound in a crystalline state, as when it was dissolved in spirits, it appeared to be decomposed. When aloine is digested for some time with strong nitric acid, much nitrous gas is evolved, and it is converted into chrysammic acid, but without the formation of any nitropicric acid, as is always the case when crude aloes is subjected to a similar treatment. A quantity of aloine was boiled with a mixture of chlorate of potash and muriatic acid. The acid solution was evaporated to dryness, and digested with strong spirits of wine. The greater portion of the spirits was removed by distillation; and the remainder, when left to spontaneous evaporation, yielded a syrup which could not be made to crystallise. Not a trace of chloranil was produced.

When aloine is destructively distilled, it yields a volatile oil of a somewhat aromatic odour, and also a good deal of resinous matter. When aloine is heated on platinum foil it melts, and then catches fire, burning with a bright yellow flame, and emitting much smoke. It leaves a somewhat difficultly combustible charcoal, which, when strongly heated, entirely disappears, not a trace of ashes being left.

A quantity of aloine dried *in vacuo* was analysed with chromate of lead in the usual way.

0·2615 grm. aloine gave 0·5695 carbonic acid and 0·104 water.
0·2415 grm. aloine gave 0·5250 carbonic acid and 0·126 water.

	Hydrated aloine Calculated numbers.		Found numbers. I.	II.
34 C............	2550·0	59·47	59·39	59·24
19 H	237·5	5·54	5·97	5·79
15 O	1500·0	35·09	34·64	34·97
	4287·5	100·00	100·00	100·00

The formula derivable from these analysis is $C_{34}\,H_{19}\,O_{15}$, which, as we shall presently see, is $= C_{34}\,H_{18}\,O_{14} + HO$, or aloine with one equivalent of water.

The aloine which had been dried *in vacuo* was next heated in the water-bath for five or six hours, and was also subjected to analysis.

Grm. Far.
0·251 aloine dried at 212° gave 0·550 carbonic acid and 0·128 water.
0·2535 aloine dried at 212° gave 0·564 carbonic acid and 0·129 water.
0·234 aloine dried at 212° gave 0·521 carbonic acid and 0·114 water.

	Calculated numbers.		I.	II.	III.
34 C............	2550	61·07	60·51	60·67	60·72
18 H............	225	5·39	5·66	5·65	5·42
14 O............	1400	33·54	33·83	33·68	33·86
	4175	100·00	100·00	100·00	100·00

The aloine employed in these analysis was prepared at three different times. These results gave $C_{34}\,H_{18}\,O_{14}$ as the formula of anhydrous aloine, that dried *in vacuo* being a hydrate with one equivalent of water.

II.—MATERIA MEDICA.

ALLEGED ADULTERATION OF BITTER BEER.

Bitter Beer, and its alleged preparation by means of Strychnine. Chemical Report by Mr THOMAS GRAHAM and A. W. HOFFMANN, Professors in London University College. London, April 26, 1852.

It is well known that for a considerable time past some agitation and discussion has been carrying on against what is called Bitter Beer, India Beer, Pale Ale, on the allegation that the bitter taste was imparted to it not by hops, but in a great degree, if not altogether, by means of strychnine. As the allegation was well calculated to diminish the consumption of this article, to reduce the sale, and, of course, to injure the trade of the brewers of bitter beer, several of these gentlemen were naturally enough displeased and alarmed, and have attempted by different methods to oppose and refute a charge which they considered to be without foundation. The subsequent report by Messrs Graham and Hoffmann, containing a chemical investigation of different specimens of Bitter Beer or Pale Ale, for home use and exportation, prepared by Messrs Allsopp of Burton-upon-Trent, was transmitted to us in the end of April; and we believe that the best way to do with this report is to publish it as it stands, since it contains information which cannot fail to be serviceable both to physicians, who prescribe the use of bitter beer, and to patients and others who may use that liquor.

Report upon the alleged Adulteration of Pale Ales by Strychnine. By Professors Graham and Hoffmann.

London, April 26, 1852.

Having undertaken, at the request of Mr Allsopp, an inquiry into the purity of bitter beer, with particular reference to its alleged adulteration by strychnine, we now submit the results which we have obtained upon the subject.

Strychnine or strychnia, the alleged substitute for the hop, is a fine crystallisable substance, extracted from *Nux vomica*, and belonging to the class of vegetable principles termed alkaloids, of which quinine from Peruvian bark, and morphine from opium, are the most familiar examples. These substances, although susceptible of the most valuable medical application in small doses, are, generally speaking, remarkable for their energy as poisons, and for the intense bitterness of their taste; two properties which are developed in strychnine in the highest degree. Half a grain of the latter substance would poison, and the bitterness of the same minute quantity is perceptible in every drop of six or eight gallons of water, in which it is dissolved.

It may be stated at once, that the quantity of strychnine which we find necessary to impart to beer the degree of bitterness possessed by pale ales, is for a gallon of beer one grain of strychnine, or double the fatal dose. The price of strychnine is about 16s. the ounce, which does not amount to so much as one penny per grain. Estimating the annual production of pale ale in Burton as 200,000 barrels, the strychnine required as a bitter would, however, amount to 16,448 ounces, and cost £13,158; while nobody believes that so much as 1000 ounces of strychnine are manufactured over the whole world. The bitterness obtained by means of strychnine is equal in degree to that of the hop, but very different in kind, and easily distinguished when the two bitters are compared. The bitter of the hop is immediate in its action upon the palate, is accompanied by a fragrant aroma, and soon passes off; whilst that of strychnine is not so instantaneous; but when the impression is once made it is more lasting, and becomes, from its persistence, like that of a metallic salt. The bitter of strychnine is, indeed, easily distinguishable from that of the hop, when deliberately tasted.

Still it would be highly desirable to be able to identify strychnine in beer by the actual extraction of the substance, and the application to it of a chemical test of absolute certainty. Fortunately those poisons which have the most violent action upon the animal economy possess often also the best marked reactions, or their physiological and chemical properties are eqnally salient. Thus arsenic and hydrocyanic acid are the most easily detected of chemical substances; and strychnine proves to be not far behind them in this respect.

A quantity of strychnine, not exceediug $\frac{1}{1000}$ of a grain, is tested and recognised to be strychnine in the following manner. The powder is moistened with a single drop of undiluted sulphuric acid, and a small fragment of chromate of potash placed in the liquid. A beautiful and most intense violet tint immediately appears at the points of contact, and is speedily diffused over the whole liquid. Although most intense, the colour disappears entirely again in a few minutes. The admixture of the smallest quantity of organic matter, however, interferes with the success of the process. In order to apply the test, in operating upon a complex liquid like beer, the strychnine must first be extracted from the liquid, and obtained in a pure or nearly pure condition. This difficulty, which appears at first considerable, may be readily surmounted, and the strychnine, if it really exist in beer, be separated, and its nature established in the most certain manner.

For this purpose, two ounces of ivory-black, or animal charcoal, were shaken in half a gallon of beer, to which ½ grain of strychnine had been purposely added. After standing over night, the liquid was found to be nearly deprived of all bitterness; the strychnine being absorbed by the charcoal. The liquid was now passed through a paper filter, upon which the charcoal containing the strychnine was collected and drained.

The next step was to separate the strychnine from the charcoal. This was readily effected by boiling the mixture for half an hour in eight ounces of ordinary spirits of wine, avoiding loss of alcohol by evaporation. The spirits which now contained the strychnine were next filtered, and afterwards submitted to distillation. A watery fluid remained behind, holding the strychnine in solution, but not sufficiently pure for the test. The final purification was accomplished by adding a few drops of potash to the watery fluid, and then shaking it with an ounce of ether. A portion of the ethereal solution evaporated upon a watch-glass, left a whitish solid matter of intense bitterness; and this was recognised to be strychnine, by giving the violet tint previously described, upon the application to it of sulphuric acid and chromate of potash.

Having satisfied ourselves by repeated experiments with samples of beer to which strychnine had been previously added, of the never-failing efficiency of the above method of extraction, we now proceeded to the actual examination of the commercial article. With this object, a series of samples were taken indiscriminately from the stores of a considerable number of the London bottlers, who supply the public with Allsopp's pale ale. We mention the beer of the following firms, whose goods were severally examined by the processes which we have above described, and from whom we have documents in hand proving the origin of the beer, and the date of its arrival at their stores from Messrs Allsopp's brewery.

*C. Barron and Co., 19 Pall Mall.
Barker and Blewitt, 211 Sloane Street.
G. and M. Blockey, Duke Street, St James's.
John Blockey, 104 Ebury Street, Pimlico.
*C. H. Bowen, 101 Fenchurch Street.

*R. B. Byass, Langbourne Chambers, Fenchurch Street.
Child and Co., Leicester Square.
L. Clow, Prince Street, Fitzroy Square.
Crimp and Ward, Old Broad Street.
Daukes and Rodick, Exeter Hall Vaults, Strand.
Nath. Eastey, 138 Upper Thames Street.
*Findlater, Mackie, and Co., 1 Upper Wellington Street, Covent-Garden.
M. B. Foster and Son, 56 Brook Street, Bond Street.
Friend and Co., Abchurch Lane.
George Jones, 8 Billiter Street.
E. and G. Hibbert, 7 Jewry Gardens, Aldgate.
W. J. Hollebone, 1 Lower Eaton Street.
W. Mabey, Lloyd's Coffeehouse, Royal Exchange.
J. Macdonald, 9½ Queenhithe, Upper Thames Street.
*Moline, Harper, and Moline, London Bridge.
Nisbet, 11 Jermyn Street.
Padget and Son, Pantechnicon.
Rose Brothers, 257 Oxford Street.
A. Suter, 65 Fenchurch Street.
J. Wallis and Co., Millbank Street, Westminster.
White and Price, Mark Lane.

It may be stated, that with the exception of the five varieties marked with an asterisk, the casks from which these samples were taken had all been received in London before the 20th of March, *i. e.*, the period when the possible use of strychnine in the manufacture of bitter beer was first brought before the English public.

Not one of these varieties of beer, when tested with the greatest scrupulousness, gave the slightest evidence of the presence of strychnine.

The charge of adulteration of beer by strychnine has been proposed in a manner so vague, that it is difficult to fix it, and try its validity. The existence of the adulteration is not alleged in any particular sample of beer, nor the practice ascribed to any individual brewer or dealer. An English journalist adopts the charge, upon the report that such an opinion is entertained and expressed by a French chemist of distinction, M. Payen, in his public lectures at Paris. From this gentleman we have since obtained explanations, which define more closely the kind of charge which was actually made by him. The late M. Pelletier, the well-known manufacturer of organic products in France, had received at one time an order for an extraordinary quantity of strychnine, of which the destination was at first unknown to him; but which he afterwards learned had been entirely exported to England, and used, as he informed M. Payen, to complete the bitter of certain kinds of beer.

We have reason to know, although it is not stated by M. Payen, that these remarks of Pelletier refer to a period ten or twelve years past; and further, although not informed of the amount of the order, we have good authority to state, that fifty or a hundred ounces would have been considered a large order for strychnine at that time. The calculation already given, shews how utterly insignificant such a supply of strychnine would be for its imagined application in the pale ale breweries. It is likewise known that the manufacture of strychnine has not been on the increase in France of late years.

M. Payen excuses his statements, on the ground that similar suspicions are conveyed in a French work, on "Alterations and Falsifications," by Chevallier, published nearly a year ago, but which have not hitherto received any formal contradiction in England. Notwithstanding the latter circumstance, our distinguished correspondent concludes by expressing his regret that he ever said, "that the fraud appeared to have been practised," although he had added the remark at the time, "that this falsification had no doubt ceased."

It thus appears that the charge which has been put into the mouth of M. Payen, was never made at all by that gentleman, so far as it applies to the practice of English brewers; and with reference to anterior times, that the charge reposes simply and exclusively upon the privately expressed opinion of a deceased chemist, the grounds of which are entirely unknown to the world, and must ever remain so.

In conclusion, it is scarcely necessary to refer to the sifting nature of the chemical examination which the beers of Messrs Allsopp's manufacture, for many months past, have been subjected to, and which establish their uncontestible purity. Indeed, no one who has witnessed, as we have done, the open manner and gigantic scale in which the operations are conducted in their establishment, could entertain the idea for a momont, that any practice involving concealment was possible. But even in the absence of all such scrutiny, the idea of strychnine being mixed with beer anywhere, or in any circumstances, involves an amount of improbability which might well dispel all suspicion on the subject.

There is an act of Henry VII. which prohibits the *adulteration* of ale by brimstone or *hops*. The place of the hop was then supplied by sage, horehound, chamomile, and other indigenous bitter plants. Since that period, the character of the national beverage must have undergone a silent revolution; for all varieties of beer, both pale and brown, now owe their distinctive properties to the hops which are boiled in the malt-infusion, and fermented along with it, as completely as wine owes its peculiar character to the grape. Substitute any other bitter for the hop, and the fermented wort would no longer be recognised as beer.

Were mere bitters all that is required, it would be easy to prove that the extract of quassia would supply a bitter which is perfectly harmless and agreeable, and infinitely less expensive than strychnine.

But the process of brewing pale ale is one in which nothing but water, the best malt and hops of the first quality are used, and is an operation of the greatest delicacy and care, which would be entirely ruined by any tampering with the materials employed. Strychnine could not fail to be rejected, from the ungrateful, metallic character of its bitterness, independent of all objections of a more serious kind. This peculiarity of taste is also calculated to betray its presence. Small, too, as the proportion of strychnine may be, which is necessary to impart the degree of bitterness of pale ale, the quantity rises, as has been seen, to a poisonous dose in half a gallon of the fluid; and as this poison is one of those which are known to accumulate in the system, its poisonous action would inevitably follow, in occasional cases, upon the consumption of much smaller portions of beer when continued for many days without intermission. The violent tetanic symptoms of poisoning by strychnine are also such as could scarcely fail to excite suspicion and alarm. Add to these disadvantages, the certainty of the means of detecting strychnine in beer by the chemical tests described above, which any medical man or practical chemist can apply, and the chance of the use of so dangerous a substance for any purpose of adulteration, becomes in the last degree improbable.

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Henry Allsopp, Esq., Brewery, Burton-on-Trent.

Since the foregoing report has been received, there has appeared in the *Lancet*, May 15, 1852, another report on the bitter beer, or pale India ale, of Messrs Allsopp and Co., and Messrs Bass and Co. This report is more ample, in some respects, than that of Messrs Graham and Hoffmann, and it contains a full analysis of the water employed in the preparation of the bitter beer, both in the establishment of Messrs Allsopp, and in that of Messrs Bass and Co. From this analysis it results that, while each imperial gallon of water employed in the brewery of Messrs Allsopp and Co., contains 65·28 grains of solid matter, this solid matter consists of sulphate of lime, carbonate of lime, choloride of sodium, sulphate of magnesia, sulphate of potash, and carbonate of magnesia, in proportions in the order now stated, with minute quantities of silica and protoxide of iron. The water employed in the establishment of Messrs Bass and Co., contains 78·44 grains of solid matter in each imperial gallon; and of this solid matter sulphate of lime forms 54·4 grains, muriate of lime 13·28 grains, carbonate of lime 9·93, and sulphate of magnesia ·83, or net one grain in the gallon, with a considerable amount of free carbonic acid, which tends to keep the saline matter in solution.

In this report also are given the saline matters contained in the liquors brewed.

In that of Messrs Allsopp and Sons, as ascertained by beer taken from the stores at Blackwall, while the imperial gallon contains 222 grains, this solid matter consists of 78 grains of alkaline sulphates, chiefly potash, 28 grains of alkaline chlorides, 14 of alkaline carbonates and phosphates, and the large amount of 102 grains of phosphate of magnesia fusible by the blow-pipe.

One imperial gallon of the beer of Messrs Bass and Co. contains 197 grains of saline matter; and of this 62 grains are alkaline sulphates, chiefly potash, 25 grains alkaline chlorides, 19 grains alkaline carbonates and phosphates, and 91 grains phosphate of lime and magnesia.

The phosphates are derived chiefly from the malt and hops employed.

It is important to observe, that the water in both cases belongs to the class called *hard*, containing sulphate and carbonate of lime. But it is further remarkable that, after the process of brewing, the sulphates and carbonates of lime disappear, and the liquor no longer resembles hard water. This change is supposed by the reporters to depend upon, and to indicate what they call, the depurating power of the lime; and they express the opinion that the presence and precipitation of this substance (lime or sulphate and carbonate of lime) in the Burton water is the main agent in rendering the beer bright and transparent. This may be the case. But it is to be observed, that a considerable proportion of lime is left in both sorts of beer, in the phosphate of lime and magnesia.

It seems at first sight singular, that water containing so much sulphate and carbonate of lime, should be capable of producing malt liquors so superior.

As to strychnine, it was manifest that the person or persons who made the assertion had no just or well-founded knowledge of the properties of that substance, sensible or physiological, and no practical knowledge of its remote effects.

Messrs Graham and Hoffmann, in their Report, make a remark which is something unfortunate in regard to the bittering effect of quassia, and its harmlessness as an ingredient in drinks. It has long been suspected and sometimes stated, we are unable to say on what grounds, that quassia was and is used by brewers for the purpose of imparting the bitter taste to certain malt liquors; and many persons believe that, in Scotland, the common bitter table beer is prepared with this drug, and considerable quantities of quassia are known to be sold by druggists to brewers. Perhaps it cannot be justly said that quassia is a very deleterious drug; but neither can it be maintained that it is altogether harmless. It is known, like all intense bitters, to affect in some degree the nervous system; and the long-continued use of it must produce a good deal of that venous plethora which tends so powerfully and directly to oppress the nervous system and impair its energy. If it be employed in the preparation of malt liquor, this should be acknowledged. If it be professed that it is not employed, then, to this profession, strict and scrupulous adherence ought to be given.

Meanwhile, since the appearance of the above reports, the medical profession and the public are in some degree bound to believe, that the pale bitter beer is prepared from malt and hops alone, without any hurtful or secret mixture. We may express the hope, that the confidence on the one side shall be met by fidelity on the other.

At the same time, though we allow that, in certain circumstances and states of the system, the bitter beer is the safest form of fermented liquor, we must be permitted to express the opinion, with all deference to Messrs Alsopp, Bass, and their brethren, that the safest of all drinks is water, without either malt or hops.

Since the forgoing account of the analyses of bitter beer has been prepared, a letter from Professor Liebig to Messrs Allsopp has been published, giving various confirmations of the correctness of the account, and also stating, from his own knowledge, his judgment of the safety of bitter beer as a beverage, and the groundlessness of the charge that it owed its bitterness to strychnine. The letter of Professor Liebig is subjoined.

"The unguarded remark of a French chemist, that the strychnine imported in England, is employed in part as a substitute for hops in the manufacture of beer, has lately spread alarm among the lovers of pale ale. Having been appealed to by you to express my opinion on this subject, which appears to me to be, in a dietetic point of view, one of considerable public interest, I now offer the following brief statement.

"About a quarter of a century ago, a brewer in Westphalia fell into the practice of adulterating his beer with *Nux vomica*, from which it is well known that strychnine is obtained. The peculiar morbid symptoms, however, which resulted from the consumption of this adulterated beer, speedily led to the detection of the fraud. The effects produced by *Nux vomica* and strychnine are so characteristic, that every medical man will readily detect their origin. The French novelist, Alexandre Dumas, has described them, though with more imagination than truth, in his romance of 'Monte Christo.' It is possible that the Westphalian case, which, from being made the subject of a criminal trial, obtained great notoriety, has given rise to the assumption, that in England the strychnine imported is used for the purpose of mixing with beer. But nobody at all acquainted with the great breweries of that country could seriously entertain the suspicion of an adulteration of beer with strychnine or any deleterious substance. It is practically impossible that any operation of a doubtful character could be carried out in these extensive establishments, on account of the large number of workmen employed in them. Any attempt on the part of the brewer to impart to his beer in an illicit manner, qualities which are not to be obtained from malt or hops, would necessarily lead to his ruin; as he would be obliged to communicate his secret to too many persons, and to employ too many accomplices. The draymen themselves, as good connoissieurs in beer, would protest against any manipulation of a suspicious character. The case has even occurred of an eminent brewer not venturing to make use of a method suggested to him, for the purpose of clearing his beer more effectually, because the addition of a new material to the wort might have induced a suspicion in the minds of his workmen that it was an illicit proceeding, and this would have endangered the good reputation which his beer enjoyed. He stated to me at the same time that no improvement could be introduced into a brewery, the object of which was not perfectly evident to every body.

"During a sojourn of several days at Burton-on-Trent, I had an opportunity of becoming intimately acquainted with the method pursued in the manufacture of pale ale. I convinced myself that the qualities of this excellent beverage depended mainly upon the care used in the selection of the best kinds of malt and hops, and upon the ingenuity exhibited in conducting the processes of mashing and fermenting. Our continental brewers have much to learn in these points to come up to the English brewers. I have no hesitation in saying that England possesses the greatest adepts in malting. I know positively, that the chief brewers of Munich, who undoubtedly produce the best beer in Germany, have gone through an apprenticeship in Burton. This may account for the predilection entertained by the general public, as well as by medical men, for these varieties of beer; for the instinct of humanity and experience appear to be as good guides in the choice of things that contribute to health and enjoyment as the profoundest philosophy.

"Professors Graham and Hofmann, in the excellent report already addressed to you upon the alleged adulteration of the pale ale by strychnine, have indicated a very simple process for detecting the most minute quantity of strychnine contained in beer. I have satisfied myself of the great convenience and accuracy of their method, and have further assured myself, by an analyses of several specimens of pale ale obtained from London houses, supplied by your establishment, of the utter groundlessness of the imputation, that this beer was poisoned with strychnine. I am positive, and am supported in my views by the concordant analyses of all chemists who have occupied themselves with the examination of beer, that the poisoning of pale ale with strychnine has never occurred. I believe I may safely add, that it never will take place; for although an ignorant brewer might be induced from interested motives to add *Nux vomica* to his beer, the word strychnine so forcibly suggests one of the most virulent poisons, that whoever has heard anything about strychnine at all, is sure to be aware of this. By adulterating his beer with strychnine, the brewer would be knowingly committing a crime which, in the present state of science, must be followed by immediate detection and punishment.

"Mr E. Merck of Darmstadt, one of the most extensive strychnine manufacturers in Europe, informs me that this substance is peculiarly adapted to destroy vermin of all kinds. In many parts of Germany, it is the popular poison for rats and mice. This fact fully accounts for the large amount of the drug that has lately been introduced into commerce.

"The specimens of your pale ale sent to me have afforded another opportunity of confirming its valuable qualities. I am myself an admirer of this beverage, and my own experience enables me to recommend it, in accordance with the opinion of the most eminent English physicians, as a very agreeable and efficient tonic, and as a general beverage both for the invalid and the robust.

"Giessen, May 6, 1852." "JUSTUS LIEBIG."

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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*An Essay on the Yellow Fever, as it has occurred in Charleston, including its origin and progress up to the present time. Read before the South Carolina Medical Association, at its Anniversary Meeting,* 1851, *and published by their request.* By THOS. Y. SIMONS, M.D., Port Physician; formerly President of the Medical Society, and Professor of Practice of Physic in the late Medical College of South Carolina; Medical Adviser and formerly Chairman of the Board of Health, &c., &c. (The Charleston Medical Journal and Review, Vol. vi., No. 6, November 1851, p. 777.)

On the present occasion, I propose to call your attention to a consideration of the Yellow Fever, as it has occurred in Charleston, in which I shall confine myself, as far as practicable, to the characteristics of the disease as exhibited among us.

In the performance of this duty, I will,

1*st*, Give the history of the Yellow Fever from its first appearance to the present time.

2*dly*, Inquire into the causes which have been supposed to produce the disease.

3*dly*, Direct attention to the former condition of Charleston, its present condition, and the system of medical police adopted.

4*thly*, The peculiar character of the disease, and the diagnosis between it and other fevers among us.

5thly, Present a succinct view of the various remedial agents adopted for its cure.

In prosecuting this subject, I shall confine myself, as far as practicable, to the experience and observations of our own physicians;—omitting what relates to other cities in our own or other countries.

The first inquiry, then, is the history of the disease.* The yellow fever, says Dr Good, as far as we have any record of its origin, was at Barbadoes, whence it spread to different West India Islands, and at length made its appearance at Boston, in North America, in 1693, to which place it was carried from Martinique, by the fleet under Admiral Wheeler.

Dr Craigie, in his interesting article on Yellow Fever, in his work on the Practice of Physic, remarks:

"It is uncertain whether yellow fever prevailed among the indigenous inhabitants of the American continent previous to the period when it was visited by Europeans. From the testimony of Alzate and several Spanish authors it appears that long before the arrival of Cortez in Mexico, a disease named Matlazahuatl, attended with hæmorrhages from the nose and mouth, and extremely fatal, used occasionally to prevail among the native Americans, and was particularly mortal, so early as 1545 and 1546—and afterwards raged epidemically in 1736–7, and 1761–2."

"Humboldt, indeed, maintains that this disease is totally different from the yellow fever, because it attacks the red or copper-coloured race, and is confined to the interior and elevated table land of Mexico at 1200 or 1300 toises = 7200 to 7800 feet above the level of the sea."

Dr Craigie does not think Humboldt's objections satisfactory. I must, however, regard them as very potent, if not conclusive. I have never yet heard of an instance of real yellow fever prevailing among the copper-coloured race, or, in another phrase, American Indians. Nor has it prevailed in any agricultural districts. It has been confined exclusively to cities, and cannot be propagated in the country, as will in our progress of investigation be shewn; and in cities, the blacks and mulattoes—for we have no Indians in cities—are almost entirely exempt. My own opinion is, that Humboldt was right as regards the disease not being yellow fever.

Dr Craigie again remarks:

"One of the earliest distinct records of the disease, is given by Echard, who informs us,—that when the expedition against Hispaniola under Venables, returned to Jamaica, they met there an enemy more severe than the Spaniards, which he says was the plague, and which, he adds, in a little time reduced their army, originally 7000, to fewer than 2000."

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* I may here remark that most of this is taken from my report to the Board of Health, as its Chairman, in 1839.

Dr Craigie regards this epidemic as yellow fever, and not plague,—in which opinion I concur. On the earlier appearance of yellow fever in America, and particularly in Charleston, it was at first regarded as the plague. Having given this general history of the first knowledge of the existence of yellow fever, we will now consider its history amongus.

Its first appearance in Charleston was in 1699 or 1700, and it was called by Dr Hewat the infectious distemper, and considered by the inhabitants as a plague. In 1703 it again occurred. It also made its appearance in 1728. That summer was uncommonly hot and dry; the beasts suffered from the want of water, and the fever raging with violence, swept off an immense number of whites and some blacks. The state of the city was truly deplorable. As the town, says Dr Hewat, depended entirely on the country for fresh provisions, the planters would suffer no person to carry supplies to it, for fear of catching the infection and bringing it to the country. The physicians knew not how to treat this uncommon disorder, which was suddenly caught and proved suddenly fatal. The calamity was so general, that few could grant assistance to their neighbours. So many funerals happening every day, while so many lay sick, that white persons sufficient for burying the dead were scarcely to be found. Though they were often interred on the same day that they died, so quick was the putrefaction, so offensive and infectious were the corpses, that even the nearest relations seemed averse from performing the necessary duties. In 1732 and 1739, it raged with so much violence that when at its height from ten to twelve died daily. It did not reappear until 1745 and 1748, and was then comparatively mild. A few cases occurred in 1753 and 1755, and then there was a cessation of the disease until 1792, with the exception of a few sporadic cases, which occur more or less every year.

"In the year 1792," says Dr Ramsay, "a new era of the yellow fever commenced. It raged in Charleston in that year and 1794, '95, '96, '97, '98, '99, 1800, 1802, 1804 and 1807. A few cases occurred in the years 1803 and 1805; in both years its victims did not exceed 59. In the years 1793 and 1808, the disease is not mentioned at all; and in the year 1806 it is only mentioned as having occurred in a very few cases under particular circumstances. In its visitations it extended from July to November, but was most rife in August and September; with a very few exceptions, chiefly children, it exclusively fell on strangers. The unseasoned negroes were not exempt from its ravages, but they escaped oftener than the white strangers; and when attacked they had the disease in a lighter degree, and if properly treated were more generally cured. Persons, both black and white, arriving from the West India Islands, enjoyed similar exemptions from the yellow fever of Charleston. In the year 1796 and 1798 it raged with the greatest violence; but since that time has considerably abated both in frequency and violence."

From 1807 to 1817 we have no records of the existence of this disease as an epidemic, although there were sporadic cases. In 1817 it prevailed with great malignancy. It again occurred in 1819, 1824, 1827, 1828, 1830, 1838, and 1839. From the statements I have thus brought to your view, it will be seen how irregular have been the visitations of this disease.

I will now present you, as far as we have any records, with the number of deaths in these respective years. In 1699, the fever which prevailed swept off a great part of the inhabitants and a number of families. It was then considered to be a plague, "though afterwards," says Dr Ramsay, "supposed to be the yellow fever." "In 1723," Dr Hewat says, "the physicians knew not how to treat the disease." In 1732, when the population must have been very small, from 8 to 12 whites died daily, which exceeds any mortality in our city since that time, although our population has greatly increased. The following facts I collect from Dr David Ramsay:

In 1799, they were . .	239 deaths.
1800, ...	184 ...
1802, ...	96 ...
1804, ...	148 ...
1807, ...	162 ...

From 1807 we have no account of the existence of the fever until 1817. I have collected the reports of the disease subsequent to that period, from the records of the Board of Health.

In 1817 there were 268 deaths, of whom were adult males 164; adult females 56; and natives under eighteen 48. 19 blacks died, of whom 14 were males and 5 females.

In 1819 there were 172 deaths; of whom 130 were adult males, 24 adult females, and 18 natives under the age of 18. There were 5 blacks, 3 males and 2 females.

In 1824 there were 236 deaths; 160 adult males, 32 females, and 44 natives under 18. No blacks died this year of the disease.

In 1827 there were 67 deaths; adult males 40, females 3, native children over fourteen 4, under that age 10; strangers' children 6. Blacks 2, who were males.

In 1828 there were 26 deaths; adult males 21, females 3, native children 1, strangers' children 1. There were no blacks.

This same year, a disease having many of the incipient symptoms of yellow fever, viz., the Dengue, prevailed as an epidemic, affecting every one, but proving only in a few instances fatal. It is probable that the prevalence of this epidemic caused the cessation of the yellow fever and limited its extent.

In 1830 there were 29 deaths; adult males 23, females 1, native children 1, strangers' children 4, blacks 1.

In 1834 there were 46 deaths; adult males 28, females 10, native children 1, strangers' children 5. No blacks.

In 1835 there were 26 deaths; adult males 16, females 5, native children 4, blacks 1.

In 1838 there were 353 deaths; adult males 281, females 30, native children 17, strangers' children 11, blacks 4, females.

In 1839 there were 134 deaths; adult males 104, females 14, native children 8, strangers' children 7, blacks 1, female. The first appearance of the disease occurred earlier this year than usual.

The yellow fever did not appear in Charleston proper, again, until 1849. But it made its appearance in 1841 in the suburbs of the city, beyond the incorporated limits. No registry having been kept of deaths, I am unable to say how far it extended. But I believe it was limited in extent, and certainly did not occur within the city.

In 1849 there were 123 deaths; 89 males, 34 females. Of whom, 65 from Ireland, 34 from Germany, 3 from England, 3 from Scotland, New York 6, Boston 1, Georgia 1, Italy 1, U. S. 2, Portugal 1, St Johns, N. B. 1, South Carolina 33, Charleston 2. Total 123.

Of the two cases in Charleston, one was a young gentleman who contracted fever from exposure on a sea island near Charleston; he was brought up to the city and died. The other was a black man.

From the condensed statements of the fever in this city which have just been made, we may deduce the following conclusions, to which I invite your attention.

The number of native children who die during the prevalence of fever is small in proportion to strangers, considering the great number who are liable, and is remarkably small in comparison with those who die of other diseases to which children are subject.

All who have not been acclimated to Charleston, are liable in the years when it prevails, to the fever, from which, however, many escape; but those from southern latitudes are much less liable than those from northern latitudes, either in America or Europe; and those from our surrounding country are much less liable than those from the upper country.

In conclusion, I may remark, that all natives arriving at the adult age are exempt from this disease, as well as those strangers who have had the disease, and not having it have been here many years.

The number of male adults is greater than that of females. There are more males liable to the disease than females, and they are more exposed to exciting causes, and more careless in their habits and mode of life.

The number of blacks who die is small, and the deaths are among those who have been brought from the country and are unaccustomed to the city; but these cases are rare, and generally mild and manageable.

The fever generally occurs at the middle or end of July, and prevails in August, September, and October, and a few cases sometimes occur in November. In 1839, it commenced in June, and the only instance of its occurring earlier, was in 1732, when it commenced in May and terminated in October. The following statistics on this point, are extracted from the records of the Board of Health.

In 1817 there were 3 deaths in July; in August 80; in September 149; in October 33; in November 3; and these were in the beginning of the month.

In 1818, the deaths in August were 55; in September 27; and in October 20.

In 1824, the deaths in August were 32; in September 145; in October 42; in November 17. The frost occurred very early this year.

In 1827, the deaths in August were 10; in September 38; in October 18.

In 1838, the deaths in August were 44; in September 219; in October 17; in November 6; which were in the first week of the month.

In 1839, there were in June 5 cases; in July 17; in August 73; in September 32; in October 9.

There were fewer cases in September than usual, for as soon as a peculiar fever resembling the Dengue appeared, the yellow fever began to disappear.

In 1849, the first case occurred on the 26th August, and to September there were 3 cases; in September 37; in October 65; in November 18; 16 occurred up to the 10th of November, and 2 subsequently died in the Poorhouse, between the 26th and 1st December.

The next question which presents itself, is, the causes which produce this disease. By some, and at an early period, most physicians attributed its prevalence to importation.

I am not disposed to enter largely into this vexed question, upon which so much has been written, and unfortunately with a partisan rather than philosophic spirit; and after all the discussions for two centuries and better, there is still discrepancy of statements and differences of opinion upon this point. Let me summarily bring to your notice the opinions of the most eminent physicians among us in Charleston, before entering upon the philosophy of this very important question.

Dr Lining, who first gave the most graphic description of this disease, remarks, "and lastly, whenever the disease appeared here, it was easily traced to some person who had lately arrived from some of the West India islands, where it was epidemical."

This eminent physician does not give us any evidence in confirmation of this sweeping proposition, and it is not unfair to conjecture that his mind was biased by the opinion that the disease was first brought to Boston by Admiral Wheeler, from Martinique, in 1693. As Dr Rush, when he recanted his opinion of the propagation of the disease by contagion, acknowledged he was led to ascribe the yellow fever primarily in Philadelphia, in 1693–4 and 97, to contagion, he was influenced by the opinion of Dr Lining and others.

Dr John Moultrie of Charleston, who wrote an admirable thesis in 1749, when he graduated in Edinburgh, and which has been translated from the Latin into German and French, disagrees with the opinion of Dr Lining, and those who advocated the introduction and spread of the disease by contagion.

I will now quote another passage from Dr Lining. Although, says the Doctor, "the infection was spread through the town, yet, if any from the country received it in the city and sickened and died on their return home, the infection spread no further, not so as to one in the same house." The same fact has ever since held true, as far as my experience of thirty years has extended, and I have never heard anything to the contrary. This fact will evidently shew that at least the contagious or infectious character differs widely from those acknowledged as contagious and infectious by the whole profession, such as smallpox for example. But there is one striking circumstance which occurred in 1841. The yellow fever occurred on the Neck or suburbs of Charleston, yet strange to say, that many who sickened were brought to the city and died, and it did not spread or generate a single case in the city.

Let us now consider other medical opinions of more modern date. Dr David Ramsay, in a letter to Dr Miller of New York, in 1800, remarks: "The disputes about the origin of yellow fever, which have agitated the Northern States, have never existed in Charleston. There is but one opinion among the physicians and inhabitants, and that is, that the disease was neither imported nor contagious."

This was the unanimous sentiment of the Medical Society, who in pursuance of it, gave their opinion to the Government last summer, that the rigid enforcement of the quarantine laws, was by no means necessary on account of yellow fever.

Now these opinions were generally entertained until 1839, the particulars of which it becomes my duty to give, and let others form their judgment. The circumstances then occurring, have awakened doubts, if not convictions, in the minds of some few very intelligent physicians, whether the yellow fever has not occurred among us through importation, and either by infection or contagion, or to use a new phrase, transmissibility. I will, therefore, give you all the facts and the opinions founded upon them, and leave it to your judgment to decide.

In 1830, the late Dr Stroble endeavoured to prove it occurred from importation, and, predicated upon this, he wrote a work to prove the transmissibility, or in other words, contagious character of the disease. I refer you to the work for particulars, and will content myself with quoting from the work on the Practice of Physic, by our esteemed and eminent associate, Prof. S. H. Dickson, for whose medical opinions generally I have much respect.

"Dr B. B. Stroble, of this city," says Dr Dickson, "struck with the early and unexpected appearance of yellow fever here in June 1839, was led to a patient, impartial, and attentive examination of the subject. For the detail of his facts and arguments I refer you to his published work. They seem to me weighty if not conclusive, and if they do not prove, they surely render highly probable, the doctrine that yellow fever is in this country and climate, as it has been long maintained to be elsewhere, contagious and communicable, or, as Dr Stroble has phrased it, transmissible, yellow fever is contagious; in other words, a case of yellow fever having been generated in a favourable season and locality, by its unknown and undetected cause, becomes itself a generating centre productive of other causes, or of a morbid agent capable of producing them.

"It is transmissible from any one centre to another, or from any one of its generating centres to a healthy locality, and this communication or extension may take place in two modes, either by consequence of a portion of atmosphere in which is diffused its undefined specific cause, as in the hold of a foul vessel, from any place where it prevails epidemically, or by the introduction of a sick body or any fomites imbued with its contagion."

Now, here we have the opinion on one side. Let me present you the other.

On the 7th of June 1839, three patients were admitted into the Marine Hospital, from the ship Burmah, which had arrived from Havannah, of which I was informed by the physician of that institution. The pilot, contrary to the regulation that all vessels having sickness on board should be brought to quarantine, brought in this vessel. The remainder of the seamen on board that were sick, were sent to the Lazaretto, and the ship was thoroughly cleaned and ventilated, she being in ballast. She was in the stream and did not come to the wharf for some weeks after, and had no communication, as far as could be ascertained, with other vessels. On the 17th and 19th, cases were admitted into the Hospital from the ships Chatham, Leonore, and Elizabeth Bruce. The Chatham and Elizabeth Bruce were lying at Fitzsimmons' wharf; the Leonore was lying in the stream, opposite these vessels, and had never been to the wharf; the Burmah was lying in the stream off Roper's wharf. The distance from each of the vessels was therefore considerable, and there were a good many vessels between, on board of which no sickness had as yet occurred. The Chatham arrived here from Boston, on the 5th of June, in ballast; the Elizabeth Bruce arrived in Charleston from New York, 7th of June, in ballast;—and the Leonore sailed from Boston and arrived on the 7th of June, in ballast; all the crews were well. Subsequently, the disease occurred in different vessels in the harbour, which it would be unnecessary here to detail.

The appearance of the disease so early in the season, and so soon after its occurrence on board of the Burmah, created a suspicion of contagion in the minds of some. But I could not, upon the minutest investigation, come to that conclusion, and a committee, consisting of Drs Lopez, Moultrie, Geddings, Campbell, Winthrop, and Horlbeck, which was appointed by the Medical Society, after making a minute and thorough investigation, came to the conclusion that the fever was not introduced by the Burmah, or by contagion, but was produced by the peculiar condition of our atmosphere. In other words, it was epidemic, and arose from causes among us.

From that report I select the following passages:

"By an examination instituted through reference to the captains and mates of the various vessels, whose information was given from the log-books, your committee have ascertained that they had, at the time of their sailing from the different ports, viz., Liverpool, Boston, and New York, for this port, perfectly healthy crews, with sound cargoes, incapable from their character of generating foul air. That there was no malignant disease prevailing at the time of their departure; that their crews had not, while in this port, any communication, either direct or indirect, with the Burmah or her crew; and finally, that *that ship*, in all these instances, lay in the stream, from a quarter to half a mile from them, except for a short period, of which mention will hereafter be made." —(Page 5.)

Again: "The presumption is thus fairly induced, that the cleansing and ventilation must have disinfected her (the Burmah) sufficiently of her foul atmosphere for purposes of safety, *else, why none other of her crew remaining on board* THENCE TO THE TIME OF DEPARTURE FROM OUR PORT, CONTRACT THE DISEASE?"—(Page 7).

Again: "Thus far your committee have satisfied themselves, that the transmission of the fever through the agency of the Burmah, is neither tenable as a fact, nor in accordance with the opinions of a great majority of the medical profession in this country."—(Page 8.)

And again: "Your committee therefore, are of opinion, that the yellow fever which has prevailed, and still continues this season, has its origin, not from contagion derivable from those cases imported in the ship Burmah on the 6th of June last, but from local and general causes."—(Page 10.)

I may here remark, that Dr Lebby, in a letter to Dr Stroble, shews that yellow fever occurred at Fort Johnson, some time before the Burmah arrived—and could not be possibly traced as arising from any foreign agency.

I may here also remark, that in 1838, a fire occurred which destroyed one-third of the city of Charleston—and in this year and 1839, the exposure of an immense mass of materials on the surface of so much uncovered earth, and water in the cellars, were exposed to solar influence sufficient of themselves to generate yellow fever.

I have thus given you the opinions of our own writers and physicians. You are all doubtless well acquainted with the different views taken by medical authors in the United States as well as Europe, and I think after a thorough investigation, you will readily perceive different conclusions drawn from the same facts, and indeed, what is more unpleasant, often discrepancy of opinion, as regards the facts. Likewise that bilious remittent fever has been not unfrequently confounded with yellow fever, and above all, in the ardour of discussion, philosophic research after truth has too often been lost in the maze of controversial sophistries. Yet, I think that the most general opinion is, that the yellow fever, when epidemic, arises from local causes; while its contagious, or rather infectious influence, under certain circumstances may occur. All epidemics may become infectious without being strictly contagious or capable of transmissibility or extension, beyond a certain limit; and hence in former ages, they were all regarded as contagious, whereas now it is more than questioned by many.

Let us now inquire into a very interesting point of medical philosophy, which may throw some light on this subject. In former times the system of medical police and medical hygiene were miserably pursued. It was then in the treatment a rule to have all fresh air excluded from the sick, for fear of catching cold. Now from this regimen, we observe that not only there was an accumulation of fomites emanating from the body, but an exhaustion of vital air, oxygen gas, and the accumulation of carbonic acid gas, (non-vital); hence the aggravated type and infectious character of the diseases then existing;* and under such a system of regimen, it is not at all surprising that those, as nurses or friends, who were exposed to such a vitiated air, should themselves feel the effects of the disease. Hence the terrible accounts which were given of the infectious and pestilential nature of epidemics, and the violence and mortality incident thereto. From chemical discoveries, and the judicious application of these discoveries to useful and practical purposes, a new system of medical police and medical hygiene became gradually to be adopted, the effects of which have been signally observed wherever they have been faithfully used. We now regard it as essentially important, to have a free ventilation to such an extent as to correct the vitiated air produced from the sources already stated, and we likewise use certain substances which are regarded at least, as partially disinfecting agents, and that plan has had a manifest influence and effect in diminishing the aggravated types of epidemics. We hear now no longer of the typhus gravior, which was so strongly depicted heretofore, which, says Southwood Smith, is not known in the hospitals; but we have had evidences of the same disease in cases of emigrants crowded on board of vessels ill-ventilated, and accumulation of filth generating a foul and pestilential atmosphere, and creating a fever very similar to the description given of the typhus gravior.

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* I may mention here an anecdote of that original and eccentric medical genius, Dr Radcliffe, in the reign of Queen Anne. He was attending the young Duke of Beaufort, in smallpox, and in opposition to the received medical opinion, as well as the strong remonstrances of the Duke's mother, ordered the windows to be opened to obtain fresh air. This was strenuously resisted. Dr Radcliffe, however, persisted, and from that period, a change in the symptoms took place, and ultimate recovery followed.

From the great improvement in medical police, the apprehension of the propagation of these fevers by infection, has been much questioned by many, and the virulence and mortality evidently greatly diminished. Proceeding upon these principles, Napoleon Bonaparte, in May 1799, performed an act which had a powerful influence as regards his undaunted courage among his soldiers, and was considered by his medical staff a proof of good sense, when he passed through, undismayed, the hospitals in Arabia, encouraging his men suffering with the plague, a disease which, like yellow fever, was regarded with as much horror as the leper in ancient times, and which made the boldest warriors shrink with terror. These hospitals were well ventilated, and the virulence and mortality were much diminished.

There can be no doubt, then, that an atmosphere can be so generated in a ship or residence, as to produce a similar disease to those in its immediate atmosphere; but whether a person or persons coming into a healthy city could propagate the disease so as to vitiate the atmosphere of a whole city, and create an epidemic, is very questionable. I will here introduce two very interesting cases I published many years ago, illustrative of this subject.

A Spanish vessel came into Charleston and anchored at the quarantine station, with all her officers sick with yellow fever. The mate of an American vessel sailing from New Orleans to Philadelphia, was placed on board the Spanish vessel while at sea, to navigate her to Charleston. The mate, when I visited her, was perfectly well; the next day, however, he was seized with yellow fever, and died on the fourth day with black vomit.

To satisfy my mind as far as practicable, I wrote to New Orleans to ascertain if yellow fever was prevailing there at that time, to which I received in answer it was not. I then wrote to the port physician of Philadelphia, to ascertain if any sickness existed on board of the vessel on its arrival at quarantine, and the reply was—" There was none."

Now, here seems to me a clear case of infectious influence.

Again, of another disease, viz., Cholera, I will cite an instance which occurred on our sea-coast, and which I published. The brig Amelia was stranded on Folly Island, about twenty miles or more from Charleston, with patients suffering from cholera. Several of those who went to the wreck and encampment near, thereby took the disease, and as soon as the vessel and all her materials were burnt, the disease became arrested. Two of the sailors absconded and arrived in Charleston, sickened with cholera in Elliott Street, and died; but no other cases occurred in the city, nor was it propagated therefrom.

Dr Dickson wrote a very graphic account of this circumstance, and it was regarded by him and the contagionists, as a most powerful evidence of its infectious character. Now, I admit and concur in the fact of its infectious influence to the extent in which it was generated. But still it is questionable if it could have extended beyond a certain limit, or, in other words, would have in a city produced a general epidemic. Be this as it may, the circumstances prove an important lesson in medical police, to prevent all cases of such kind being introduced into a city. From what I have presented to you, you will perceive I have endeavoured to do so in the most impartial manner. You will likewise perceive that I regard it as peculiarly essential to have a judicious regulation of external and internal medical police. The laws of epidemics are puzzling problems in medical research, and have not been satisfactorily explained either by chemical analysis or meteorological observations. Yet the investigations in these respective departments of scientific research have led to much valuable and useful improvements in medical police, if not in preventing, at least greatly ameliorating, the mortality by epidemic disease.

I will now consider the condition of Charleston formerly, and its present condition, and the consequent progressive improvement in the medical police. The following extract is from Dr Ramsay's History of South Carolina, a work now out of print. The extract is long, but I hope I will be excused in presenting it.

" George Chalmers, in his Political Annals of the United Colonies, printed in 1780, pp. 541, 542, observes that ' Charleston was long unhealthful. From the month of June to October, the Courts of Justice were commonly shut up.

No public business was transacted. Men fled from it as from a pestilence, and orders were given to inquire for situations more friendly to health.' This statement is corroborated by tradition from the elder citizens, who inform us that in the time of their fathers, the sick were sent from Charleston, to expedite their recovery in the more wholesome air of the country; and that the country was preferred, on the score of health, as a summer residence. This is by no means improbable. The situation of Charleston in its natural state, was a slip of land stretching southerly between the two rivers, and projecting into the harbour formed by their junction, and divided into a number of peninsulas by creeks and marshes indenting it on three sides, so as to leave but little unbroken high land in the middle. The first buildings extended along East Bay Street, and had a marsh in their whole front. A considerable creek, named Vanderhorst's Creek, occupied the foundation of Water Street, and passing beyond Meeting Street, sent out a branch to the northward, nearly to the Presbyterian Church. Another creek stretched north-westwardly nearly parallel to East Bay Street, from the neighbourhood of Macleod's lots, through Longitude Lane and to the north of it. The same kind of low ground ran up Queen Street, then called Dork Street, beyond the French Church and through Beresford's Alley, till it approached Meeting Street. The north end of Union Street was planted with rice about the middle of the eighteenth century. Another very large creek occupied the site of the present central market, and extended westwardly beyond Meeting Street, which diverged southwardly almost to the Independent Church, and northwardly, spreading extensively, and dividing into two branches, running to the north-west and to the north-east, so as to cover a large portion of ground. Besides the marsh and these three creeks, which nearly environed three sides of the improved part of Charleston, there was a fourth creek a little to the southward of what is now Water Street, which stretched westwardly over Church Street; and a fifth, which ran northwardly up Meeting Street, and then extended across westwardly nearly to King Street. A creek ran from the west, near where Peter Smith's house now stands, and nearly parallel to South Bay, till it approached the last-mentioned creek, and was divided from it by King Street and a slip of land on each side. Six other creeks ran eastwardly from Ashley river, three of which stretched across the peninsula so as to approximate to King Street.

"Besides the creeks now mentioned there were also ponds and low grounds in different parts of the town. One of these extended on the east side of King Street, almost the whole distance between Broad and Tradd Streets. This was granted to the French church in 1701, but being useless in its then state, was leased out by them for fifty years. In the course of that period the tenants improved and built upon it. There was also a large body of low grounds at the intersection of Hasell and Meeting Streets. The elder inhabitants often mention a large pond where the Court-House now stands. It is believed that this, though real, was artificial. It is probable that the entrenchments attached to the western fortifications of Charleston, which extended up and down Meeting Street, from the vicinity of the Independent Church to the vicinity of the Presbyterian Church, were dug so deep as to cause a constant large collection of water at that middle part of the lines. It was the site of Johnson's covered half moon, and of a drawbridge, over which was the chief communication between the town and the country. No prudent engineer would erect such works as these on a pond, though when they were erected in the soft soil of Charleston, they would be very likely to produce one. Whether this was a natural or artificial collection of water, there was enough in other parts of the town to make it unhealthy. Such, with some small alteration, was the situation of Charleston for the first seventy years after its settlement.

"To reduce such a quagmire as a great part of Charleston originally was, to a firm, high, and dry state, required time, labour, and expense. Much has been done, but much remains for future enterprise.

"The pond at the south end of Meeting Street, was filled up and built upon by Josiah Smith, in the years 1767, 1768, and 1769, at an expense of about £1200 sterling.

"Vanderhorst's creek was turned into firm solid land, between the year 1788 and 1792, and obtained the name of Water Street.

"The creek running under the governor's bridge, was finally obliterated and turned into a market, between the years 1804 and 1807. The extensive marsh land and low ground to the north and west of this creek, had been filled up and built upon some years before, by John Eberly, Anthony Toomer, and others. The time when the other creeks were converted into solid land and improved, cannot be exactly ascertained."

Such, then, was the condition of Charleston when it was early settled. Gradually and progressively, all of these ponds and creeks were filled up. Subterraneous drains in the different streets were made, and drains from the yards of respective premises were made to communicate with the drains in the streets. But it was found that the drains having the grates in the centre of the street, and becoming thus filled up, caused, after heavy rains, a vast quantity of water to be accumulated in the streets, making them at times almost impassable. The offals of the city were required regularly every day, except Sunday, to be removed.

The first improvement was the paving of East Bay Street and Elliott Street, where the yellow fever was generally the most rife; a great improvement in the healthiness of these localities was found to be the result. The great and general improvement, however, commenced in the administration of General Hayne, in 1836, at which time I was a member of the Council. It was then determined to make a general system of internal medical police. The leading objects were the following.

1st, That every low lot should be thoroughly drained, so as not to allow water to remain thereon and become stagnant, and if they could not be drained to have them filled up.

2d, To have all the cellars existing to be kept so constructed as not to allow accumulation of water within them, and if this could not be done to have them filled up.

3d, To forbid the formation of subterraneous cellars in all subsequent buildings.

4th, A great and important object was adopted, to have the streets paved or macadamized convexly, and to have side drains in the streets communicating with the main drains in the streets, thus preventing the accumulation of water; and having likewise the drains thus cleansed, and thereby preventing the accumulation of filth, requiring them to be opened and cleansed,—a great cause, in my opinion, of disease.

Now these plans have been progressively going on, and in proportion has the healthiness of our city been improved; and while the public authorities are gradually, nay, I may say, actively pursuing this plan, I firmly believe it will be the cause of making the city not only one of the healthiest among commercial cities, but may possibly make us in a great degree, if not entirely, exempt from yellow fever, and should it occur, lessen its virulence and mortality. From long experience and observation, I regard it the solemn duty of the public authorities to go on with this plan, regardless of expense, not only for the preservation of health, but for the extension of commercial prosperity.

Let me now say what is generally admitted; that the exhalation of heat without moisture or accumulated vapour, as exhibited by the dew-point, will not produce yellow fever any more than moisture without high temperature will do it.

It is only in more modern times that we have had proper meteorological observations. The thermometrical heat among us was formerly given, but the hygrometrical and dew-point were not understood, nor had we electrometers. Hence, in 1728, we are informed that the summer was hot and dry, meaning there was no rain ; yet, I have no doubt the atmosphere was damp and the dews heavy. Such, at least, is my experience in dry seasons among us.

From the statements which I have brought to your view, you will readily perceive, that while I regard it as proper to prevent, as far as practicable, the introduction of yellow fever from other sources, we should not be forgetful to remove all causes which may arise among us to produce the disease. It is a great error to conceive that we receive all the evils of epidemics from other sources, rather than causes among ourselves. The true, and in my opinion, the wise plan, is to attend particularly to internal medical police, with a judicious care as regards introduction of the disease from other sources. I must in candour say, that I do not believe the yellow fever was ever produced in Charleston from importation ; and this is the result of an experience of thirty years as Port Physician, and twenty-four years a member of the Board of Health, many years of which I have been its chairman.

We will now describe yellow fever:

1*st*, To give its peculiar characters.

2*d*, The different subordinate forms which it exhibits.

1*st*, Yellow fever is a continued fever for three days, sometimes less, which may be called its first stadium or paroxysm.

The fever comparatively subsides after this period, or we have the commencement of black vomit. There is in the first stage much irritability of the stomach and pain in the epigastric region, which, if not controlled, produces in the second stage an ejection first of a greyish flocculent matter, accompanied with viscid mucus; and if this is not arrested, the black vomit, resembling coffee grounds, is thrown up. In this stage sometimes, there is bleeding from the nose, gums, and ears, and even the finger nails, with vomiting of venous blood, previous to black vomit. The circulation becomes now languid, the skin cool, and in many places of a bluish mottled appearance, especially along the spinal column, probably from the pressure in remaining in a recumbent position. There are heavy and deep sighings, sometimes hiccoughs, and a slow and laborious breathing. These, then, are the general phenomena presented, according very much with what were faithfully described by Dr Lining, as far back as 1748, shewing no change in the character of the disease in this long interval of time. Having given these general symptoms as I have observed them, we will proceed,

2d, To the varieties which are presented. These depend upon the constitutional temperament of the individuals attacked. Thus in the sanguineous and bilio-sanguineous, we find a high exaltation of nervous and circulatory excitement; generally in the latter, a higher degree of circulatory action than in the former. In these cases there are always a strong action of the heart and a strong pulse and active capillary circulation, exhibiting the following phenomena: quickness of respiration and great uneasiness, and even acute pain in the epigastric region, increased animal heat in the cutaneous surface, with redness of tongue and excessive thirst. The cerebral excitement is great, accompanied in some instances with delirium, and if not, with violent pain in the head, in the cerebellic portion of the brain, and the spinal column; there is likewise irritability of the stomach, a diminution in the secretions of the liver, kidneys, and cutaneous surface, and frequently in the mucous membranes, as evinced by the dryness of the tongue. I have seen some cases where the intensity has been concentrated in the brain and cerebro-spinal column, in which there is intense agony, with symptoms of the character of phrenitis. In such cases the extremities are very cold.

Now there are cases again where the general symptoms are directly the reverse, and these are in phlegmatic temperaments. In such cases there is a languid circulation, the pulse being slow and easily compressed, a slow respiration, a languor of cerebral functions, with a torpidity of mind and drowsiness, a pale and cool skin, having at times a mottled appearance, and a diminution of animal heat, evincing a feeble capillary action. The other symptoms are not dissimilar to those already described. These two different forms are the inflammatory and the congestive, in both of which the characteristic diagnosis of yellow fever is maintained; and in both these forms will be found, generally speaking, a striated redess in the tunica conjunctiva, gradually advancing in the progress of the disease (*pari passu*) with the skin becoming yellow.

We are next to consider the prognosis of the disease. I regard this in many instances difficult. If the various symptoms which have been already described, are not relieved, and the irritability of the stomach continues with the ejection of black vomit, the prognosis is unquestionably unfavourable, although there have been extraordinary cases of recovery under these inauspicious circumstances. A heavy and deep sighing, with laborious breathing, in all cases wherever the other phenomena are favourable, is in this, as in other fevers, a dangerous indication. If these indications are relieved and the patient has biliary evacuations, and all the active derangements are composed after the fourth day, the prospects of recovery are cheering, although there have been many melancholy and fatal changes.

It is, in truth, a very difficult matter in all cases, however seemingly favourable, to make a prognosis ; and the patient should always be regarded as in a precarious condition until all the unfavourable symptoms disappear.

I will now consider briefly the diagnosis of the disease. The fever with which it has been frequently assimilated, is the bilious remittent fever, and hence by some it has been called the paludal fever,—upon the supposition that it was produced by marsh miasmata.

1*st*, Yellow fever is a continued fever of one paroxysm and different stadia, as already described. Remittent fever has, as it name imports, daily remissions and exacerbations.

2*d*, Yellow fever prevails in cities, and cannot be propagated in agricultural districts, and rarely is known in cities upon navigable rivers, far remote from the ocean; although there are exceptions to this rule ; but I think it was frequently the bilious remittent fever of an aggravated type, in place of yellow fever. Oftentimes physicians thus fall into the same error as many Europeans, from not examining the vomit ejected. Remittent fever is peculiarly endemic in agricultural countries in warm latitudes.

3*d*, Once having had the yellow fever, is an exemption from future invasions. In remittent fever, once having it, so far from being an exemption, renders one more susceptible to the malaria influence.

Such, then, are the prominent diagnostics. The peculiar yellowness of the skin, in the progress of yellow fever, resembling jaundice and its type, being of continued character, induced Sauvages and Cullen, two celebrated nosologists, to give the following terms to the disease. Sauvages denominates it *typhus icterodes* ; Cullen, *typhus cum flavedine cutis*, a distinction without a difference. The yellowness of the skin is not a diagnostic symptom of yellow fever, as it is almost equally an accompanying symptom of bilious remittent fever. Nor can it be regarded as a typhus, as yellow fever requires altitude of temperature, and is the disease of warmer climates ; the other of diminution of temperature, and is the disease of cold climates. The black vomit, the *vomito prieto* of the Spaniards, has been regarded as a diagnostic sign, although black vomit has occurred in other cases. Yet this symptom is comparatively with few exceptions in this disease,—the termination of the first stage. There is often ejected in bilious remittent fever, a kind of black atrabilious matter, which has, upon ordinary inspection, the exact appearance of the black vomit of yellow fever, but subjected to minute examination, will be found widely different. The opinion of Dr Rush, and some others, was, that yellow fever was nothing more than an aggravated type of bilious fever, and that the black vomit was nothing more than vitiated bile. Dr Physick, however, made many interesting autopsies, and clearly proved that in all the cases, with few exceptions, the liver was in a healthy condition, and no vitiated bile in the gall bladder; that the stomach was the centre of the disease, shewing abrasions, congestions, &c. &c., and he demonstrated the difference between vitiated bile and black vomit. Subsequent anatomical investigations have confirmed his views. Many years since, when I was Physician to the Alms House Hospital, I collected the vomitings from those affected with bilious fever, and those with yellow fever, and as Professor of Practice of Physic, demonstrated clearly the difference between them. In repose, they could hardly be distinguished; agitated and placed on paper, the vomit of the bilious remittent patient would shew the marks of bile; the yellow fever, the same coffee-ground appearance untinged. These were mere confirmations of Dr Physick's previous experiments.

You are well aware, that there are various physiological theories and speculations as regards the character of the gastric juice; among others, that it contains free muriatic acid. At that time, when as Professor of Practice of Physic I lectured on yellow fever, I made experiments before the class, of fresh-drawn venous blood, to which I added diluted muriatic acid gas, and after shaking it, there was all the appearance of the black vomit; and when tested in every way, it could not be distinguished from the black vomit which I had preserved; both being exhibited at the same time. Those who are curious can repeat the experiments. It is evident, therefore, that a great mistake has been made as to the character of the ejections of the stomach, in bilious remittent and yellow fever, by very many medical gentlemen of high eminence. It would be tedious and supererogatory to detain you longer, or enter more minutely into the pathological character of yellow fever.

We are now to consider the treatment of this disease. In this, as in other diseases, great diversity of opinion prevails. From the description of the disease, and its modifications, you may readily perceive that we cannot depend exclusively upon any one regular course of treatment. The American and English physicians, generally, regard calomel as the *sine qua non*, or sheet anchor, to be used so as to induce ptyalism.

This the French and Spaniards condemn as prejudicial and fatal, and give mild laxatives, the warm bath, ptisans, &c. The active purgation with seneka snake root and Epsom salts has been frequently used. There are others who regard calomel to a certain extent valuable, but cannot agree in the opinion that calomel is a specific, or *sine qua non*, in the disease. They regard it valuable as exciting biliary secretion; for the liver in this disease is torpid, and the production of biliary evacuations is regarded as a very favourable and important symptom.

In the treatment of the symptoms, blistering or cupping on the epigastric region, mustard pediluvium, if the extremities are cold, cupping or leeches to the head and spine, or general bleeding, cold affusion and warm baths, have all been used; and these applications must be regulated by the temperament, and in accordance with the symptoms developed, and according as the disease is inflammatory or congestive. This general view is all that is regarded as necessary on this occasion.

I will now present you briefly with the plan I have generally adopted in the treatment of this disease, referring you to the different works published of the different views taken of the treatment by others. From the different forms in which I have described as inflammatory and congestive, you will readily perceive it would require a different course of treatment. In the inflammatory form, I generally employ bloodletting, so as to make a strong impression. I also give a large dose of calomel, say twenty grains, and in four hours' time give seneka snake root and Epsom salts until it operates well, and have the spine and limbs bathed with spirits and cold water frequently.

After the depletion by bleeding and purgation, if the pain in the epigastric region is not diminished, and if there is irritability of stomach, I immediately place on that region a large blister, and keep the irritation there continually, so as to act as a powerful counter-irritant. If the pain in the head and in the spine continues, I use cupping and leeches. Of medicines, I now give four grains of calomel, with some *creta preparata* to keep up biliary secretion and subdue acidity of the stomach,—a very important object,—with an occasional injection *per anum*; and I regard injections very valuable as a kind of derivative from the stomach, and preventing the stomach, so peculiarly affected in this disease, from being irritated by cathartic medicine. Such, then, is the course of practice which I have followed in the inflammatory form. When the extremities are cold, mustard poultices or any other stimulating application are then applied as a derivative.

The next form is the congestive, the symptoms of which

I have described. As in the inflammatory, the great object is to bring the over-excited condition of the system to such a state as to prevent congestion in the capillary system, and derangement of functional action in the different organs, so in the congestive form we must endeavour to arouse the system from the extreme depression which exists. For this purpose, whenever practicable, I use warm stimulating baths; and if not convenient, I apply mustard plasters to the stomach and extremities, and sponge with stimulating applications. The great indication is to arouse the nervous system, and to have a proper circulating action. The other means of practice are not dissimilar to what I have described, so far as medicine is concerned, in the inflammatory form.

These, then, are the plans in the first paroxysm or stadium.

I will now briefly consider the treatment in the second stage, but previously remark that the great class of patients are of that population where it is difficult to carry out faithfully a judicious course of practice, except when in hospitals; and hence the mortality. Here, I frankly confess, all our treatment is empirical. The stomach, in the stage of black vomit, is evidently not only functionally but organically deranged. There is hypertrophy of the Vasa Brevia, and an exudation of blood incident thereto, as I have previously explained. In this stage, yeast, sugar of lead,* effervescence of carbonate of soda and tartaric acid, carbonic acid gas, and many other means of this kind, have been used. My opinion is that no medicament in this stage can be of any service as taken in the stomach. The only plan which may be probably advantageous, is to keep up in the epigastric region a constant irritation, and to give, according to circumstances, stimulating injections.

I have thus endeavoured, in a very humble way, to discharge the duty assigned me, and, as in my introductory address†, I repeat, I could have wished the appointment as your orator had been given to an abler member. Should it awaken a proper feeling to investigate the peculiar characteristics of our endemics, I should, indeed, be more than gratified. I have now only to invoke the blessings of the Great Supreme upon the labours of our Association, the objects of which are to produce brotherly love, and promote the cause of medical science and humanity.

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* Our late very eminent physician, Dr Irvine, wrote a very interesting article on yellow fever, in which he advocated the use of acetate of lead, to act as a sedative and subdue the irritability of the stomach, and prevent the functional derangements incident thereto. But his theory was not sustained by practice.

† This part of the Address was omitted as unnecessary, but will appear in the proceedings of the Association.

ART. II.—*Observations on the Medical Topography and Diseases (especially Diarrhœa) of the Sacramento Valley, California, during the years* 1849, 1850. By J. D. B. STILLMAN, M.D., of New York. (The New York Journal of Medicine.)

[No good account has hitherto been published of the diseases prevalent in California, although all the newspapers have given statements, shewing that the Valley of the Sacramento is a very unhealthy district, and has caused great mortality among those who have visited that region. The following account, by Dr Stillman of New York, will be perused with some interest, as coming from a physician who has witnessed the scenes which he describes, and the facts which he records.]

The emigration which took place from the United States to California, in the year subsequent to the discovery of gold in that country, will be remembered as one of the most remarkable events of this century. If we consider the character and number of the emigrants, the distance traversed, the hardships and privations endured, and the magnificent results attained, the event has no parallel in history.

The number who arrived in California during the six months from 1st July 1849 to 1st January 1850, was above 90,000. Of these nearly 30,000 performed a voyage by sea of 17,000 miles; more than 60,000 crossed a wilderness of greater extent than the entire distance from the mouth of the Tagus to the eastern confines of Russia, over arid plains and rugged mountains. Of this number, it was roughly estimated that one-fifth had found graves within the first six months after their arrival. An investigation of the circumstances that conspired to such a result constitutes a subject of extreme interest and importance.

No effort has been made by medical men conversant with the facts to give them to the public, that I am aware of; and as the most confused and conflicting views were entertained with regard to the nature and origin of the diseases that caused such remarkable fatality, I have been induced to give the results of my own observation, made during the summer months of 1849, in the Sacramento Valley.

The chaotic state in which society was thrown together during the first few months of its existence, was very unfavourable for statistical information. It was not until about the beginning of the year 1850 that any attempt was made, even at Sacramento city, by the temporary government, to keep a record of the deaths, simply, and that was so imperfect as to be of no value. The opinions of medical men were contradictory ; and an unfortunate tendency to exaggeration, and a loose method of generalising facts, made it difficult to arrive at any conclusions further than were forced upon each observer by his own observation. The climate and physical features of the country are peculiar ; and it was but natural that the most extravagant accounts of anomalous and endemic diseases should gain the public ear.

The month of August was spent in an encampment, at a place now included in the thickly-settled part of San Francisco, known by the misnomer of "Happy Valley," and close to the shore of the bay. On the west and northwest rose a succession of sand-hills, covered chiefly with shrub oaks, and extending to the sea-shore. Passing over a low sand-hill on the southwest, at the distance of two or three miles, lay the old mission grounds of Dolores. Eastward from these is an extensive flat, partially flooded at high tide, and never dry. This place, with the immediate neighbourhood, was occupied exclusively by the tents of those landing from vessels just arrived by the way of Cape Horn, preparatory to starting for the interior. The most of them were in robust health when landed, having had a good supply of wholesome food. Some vessels, however, were not supplied with vegetables, and from these a considerable number of cases of scurvy were landed who took their quarters amongst us. Of these several died.

During that month the weather varied but little from day to day, though between noon and night the temperature varied greatly. The mornings were cold and foggy. About ten o'clock, A.M., the sun shone out clear ; and at one o'clock, P.M., the thermometer in my tent stood from 85° to 90°. About two o'clock, P.M., a breeze sprung up, cold and strong, from the west, accompanied by dense masses of fog ; and by four o'clock, the temperature would frequently have fallen forty degrees. This extreme coldness of the sea-breeze is occasioned by the ocean currents, which, flowing northward along the coast of Asia, set against the northwest coast of America, and return southerly, keeping a current of cold water on the coast of California.

Tents furnished but a poor protection from the heat of the sun, closed as they were on three sides to keep out the sand and dust. I took the pains to ascertain the number of men living in these tents, and calculated that the average population within a half a mile was about 1200. While there, I saw no cases of fever, except among persons who had been into the interior. Dysentery was so prevalent that few escaped an attack who spent a few days at "Happy Valley," or vicinity. Sometimes it was preceded by diarrhœa, but generally commenced with mucous or muco-sanguinolent discharges. In some cases blood, mixed with so little mucus as scarcely to be perceptible, was evacuated in alarming quantities. There was great dejection of spirits; and when the disease continued long it was attended by pyrexia, dry tongue and skin, thirst, and generally the symptoms that distinguished acute dysentery on the Atlantic coast. It was usually of short duration;—from four to eight days;—and was by no means a fatal epidemic. Few deaths occurred, as the patients generally recovered readily under the ordinary treatment.

The scorbutic patients seemed no more than others disposed to this affection. None that fell to my care had diarrhœa. Those who died from scurvy had neither dysentery nor diarrhœa. I have reason to believe that those who did not remain on the coast until their convalescence, did not do so well. Impatient of delay, many pushed on into the mines, where the excessive heat and fatigue, conjoined with the deprivations which increased with the distance inland, caused a greater mortality.

The water used was obtained from wells two or three feet deep, and was brackish. It was considered by many as the exciting cause of this disease, and brandy was mingled with it *ad libitum*, to improve it. But dysentery was not confined to those who used this water. The disorder occurred on board of vessels in the harbour, where the water used was brought from New York and Boston. All authors agree in making sudden transitions of heat and cold one of the most active causes of acute dysentery; and it may not be necessary to assign any other in this case. Seamen are particularly disposed to it on their return to port after long voyages, and from the equable temperature of the ocean to the variations common to the land.

The months of September and October were spent in an attempt to reach the head waters of the Sacramento in an open boat.

The delta at the mouths of the Sacramento and San Joaquim rivers is extensive, and during the greater part of the year so wet as to be impenetrable, except through the numerous streams that anastomose in every possible direction like network; and, excepting the margins of the streams, which are a little more elevated, and skirted with trees, the whole surface is overgrown with a giant rush; (*Scirpus lacustris*). At this season of the year the river was at its lowest stage. Having proceeded thirty or forty miles from its mouth, and uncertain of our way, we landed, to take a survey. But from the top of a sycamore, nothing was visible beyond a vast field of dried rushes, and a long line of foliage, marking the course of the river. The ground was dry and hard, with a few deep tracks made during the wet season by wild animals. These marshes for the most part recede from the river as we ascend, and the banks become higher, but extend far up on that side between the river and the coast range, receiving the entire drainage of the eastermost slope of these mountains. They are a favourite resort of water-fowl, which frequent them during the wet season in immense numbers.

When the water is low, the current of the river is sluggish, and the tide flows up as far as the American Fork, at the junction of which the city of Sacramento is located, and at which place the water is deep enough for large ships at all seasons. Above this it continues of the very uniform width of a third of a mile, to the Feather river, its second and most considerable tributary on the east, thirty miles above Sacramento. For one hundred and fifty miles above, it flows with a current gradually increasing as we ascend, through banks more and more elevated, but not proportionably diminishing in width and quantity of water, as the tributaries above this are inconsiderable, barely making good the loss by absorption. As is usual with rivers flowing through an alluvial plain, it is exceedingly sinuous.

The water of this river is the only water used to any great extent in its vicinity;—that from wells being highly impregnated with saline ingredients, and often offensive in taste and odour. The water of the rivers is formed chiefly by the dissolving snows, and is remarkably sweet and pure. I know of no analysis having been made of it; but from its dissolving nitrate of silver without forming a cloud, and being readily miscible with soap, I conclude that the chlorides and sulphates, the most common saline ingredients in spring water, are not contained in it in any great quantity. Its freedom from decayed vegetable matter is evident from the fact, that the water never flows from the marshes into the river, but *vice versa.* The waters of the American and Feather rivers, as they discharge into the Sacramento, are limpid and cold, as is also the water of the Sacramento itself in the upper valley; and it may safely challenge comparison on this point, even at its mouth, with any stream of its size that flows into the Atlantic ocean.

No rain had fallen since the preceding April, and the surface of the earth was dry, and often fissured to the depth of several feet. The tule beds, as well as the prairies and uplands, were barren and brown, as though scorched by subterranean fires. A blue haze shut out at all times the view of the mountains. There were no clouds to veil the sun, or moisture in the earth to take up the free caloric, and distant objects seemed to dance in the heated air that rose trembling as from a brick-kiln. This is in fact the winter of California, the season for vegetable repose; and the whole valley, except along the rivers,—where a belt of oaks, sycamores, and a few cotton woods are yet green, and a dense undergrowth of vines, willows, and coarse grass on the low bottoms, draw the half-famished birds,—is abandoned by animal as well as vegetable life, and is as much a desert as the most steril regions of the world. Even insects one looks for in vain. Owing to the extreme dryness of the air, the perspiration of the body is evaporated so rapidly, that the heat is less oppressive than the atmosphere of New York frequently is at 82°, the temperature at the time I am writing this.

The meteorology of the country is interesting, from the slight disturbance that takes place in its hygrometrical state and electrical equilibrium, during the time that the Atlantic coast in the same latitude is convulsed by thunder-storms. I know of no tables kept that year even of the temperature, and the attempts made the following year were kept so carelessly as to be very contradictory. Dew is formed in the driest season but slightly along the rivers, and on the prairie not at all. It is very copious in May and June, but diminishes as the dry season advances.

The Butes, a cluster of volcanic peaks, 800 feet high, rise out of the plain between the Sacramento and Feather rivers, like islands from the sea, and serve as a landmark for all that region. They were a resort for wild animals during the season of inundation, and were the first high ground we had seen since losing sight of Mount Diabolo on the bay. Their base is strewn with the bones of animals which have fled here far safety, and probably perished with hunger.

The dryness of the atmosphere of California admits of the same solution as that of the coast of Peru, and other parts of the west coast of both North and South America, to the leeward of very elevated ranges of mountains. The prevailing winds of summer are from NE. to NNE., as was shewn by the smoke of extensive fires on the mountains, and the head-winds on approaching San Francisco from the southwest. In the valley, however, they are rarely felt. In other words, the trade-winds prevail as far north as this at that season, and in the passage of the Sierra Nevada are driven into a region of cold sufficient to condense the vapour with which they are charged. Near the surface of the earth a countervailing force arises from the great heat of the land, and the sea-breeze on the coast blows strongly during the latter part of the day, and diffuses itself up the valley in a gentle zephyr that is exceedingly refreshing.

The first rain came about the beginning of October, and the temperature fell so low that fires were comfortable. It was preceded by a strange sickly hue, as the smoke and vapours of the valley rolled away before the changing wind, and almost obscured the sun. We were at this time encamped at Deer Creek, near Lawson's Ranch, and our party of nine, who had suffered some already, were now all subjects of the intermittent, except the cook, a coloured man. From this place we pursued our route by land, having already ascended 150 miles by water farther than Wilkes' exploring party reported the river navigable for boats. Great numbers of immigrants arriving from across the plains, were encamped in the vicinity; and though many were affected with scurvy, and nearly all were worn down by hardships, there was but little sickness among them when they came in. Many of these were directing their course with us to the head waters of the Sacramento; more to the Feather river. We met many parties who had preceded us, returning with more or less of their number sick with intermittents, a few with remittents. As we advanced, the numbers of the sick increased, and at every watering-place were many unable to continue their journey or look after their cattle, and dependent upon those passing for water, whilst every waggon was encumbered with those who were unable to walk.

Fifty miles above Lawson's, the valley loses its level alluvial character, and the hills which had been gradually encroaching at length close in upon the river. Lieut. Emmons, who was attached to the United States Exploring Expedition, and descended from Oregon across the mountains in a corresponding season of the year 1841, represents this section as the most worthless he had met with. The soil consists of gravel, coarse pebbles, and large stones mixed with sand. The most of the gentlemen connected with his party suffered from fever and ague before they reached this point.* The river here was roaring among the rocks at the bottom of a precipitous gulf, full fifty feet deep; yet even here were evidences of overflow. We were compelled to return without having gone as far as we intended, in consequence of increasing disability, and the dismal accounts that reached us from above. Dr Edwards Hall, formerly Physician to the Children's Hospital on Randal's Island, who chose a more expeditious mode of conveyance, stated to me that the settlement at Reading's Springs, about twenty-five miles higher up, was little better than a fever hospital.

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* Wilkes' Exploring Expedition, vol. v., pp. 234–244.

The second rains fell about the 1st November, and followed each other so frequently, that in a few days the roads were nearly impassable, from mire and water. The rains of the winter were not so constant or violent as to be remarkable, and had the population been properly provided with shelter, the season would not have been called unpleasant. The sun shone out spring-like a large part of the time. Ice was formed but once; that was in December. With the November rains the grass began to shoot. More rain fell that winter than usual, but its effects on the rivers were less than might have been expected. It was not until a few unusually warm days in January melted the snow upon the mountains, and sent down vast quantities of water, that the valley was overflowed. The inundation was so sudden that persons with difficulty escaped with their lives. The most of the cattle brought over the plains were drowned, and great distress caused to the immigrants. Usually the rivers do not overflow their banks until about the close of the rainy season, in March or April, or after the sun has crossed the vernal equinox. At such times it relieves itself by innumerable bayous through the levee, filling the immense plains on either side.

I ascended the Sacramento and Feather rivers again in April. Both sides of the Sacramento, as far as the eye could reach from the deck of a steamer, were under water, except the levee, as high up as the Feather river, and the whole space between the forks to an unknown distance was in the same situation. The water in the river was full twenty-five feet higher than in the previous fall (autumn), and flowing with a rapid current, but did not contain half of the water, forced as it was from its channel, and spreading out like a vast sea to the bay. Under such circumstances, the amount of water required to overflow the levee itself must evidently be immense. At this time the higher rolling grounds towards the hills, where the floods never rest long, even when they overflow them, and which in autumn were so brown and barren, are covered with a carpet of the gayest flowers. One may journey all day through an uninterrupted blaze of gaudy colours, in which yellow painfully predominates. The air is pure and bracing, and so transparent that you may see the summits of the Sierra Nevada dazzling in their coldness and grandeur a hundred miles distant. Imagination can scarcely paint a scene of more enchanting beauty than these plains at this season, and sickness is a thing almost unknown, under whatever exposure.

I have given all the details that appeared to me important in the topography of that part of California which is the subject of my sketch, that can be supposed to have had a necessary influence upon the health of the immigrants. The vegetation is too scanty to furnish the highly concentrated or putrid miasm of the tropic; but that the whole valley contains, in an eminent degree, the intermittent malaria, there can be no doubt; and that it will continue so until the greater part of its surface shall be renovated by the plough is my confident persuasion.

I took up my residence at Sacramento about the 1st of November, just as the rains had fairly set in. The condition of the great mass of the people was very uncomfortable. The principal food that had been used since their arrival in the country consisted of salt pork, hard bread, flour, fresh beef, and venison; dried fruits and potatoes could be had at a high price, and grapes grew wild along the banks of the rivers in great numbers. But, from ignorance and mistaken economy, the greater part used no vegetables, the importance of them not being sufficiently understood; and the population of the mining regions were almost entirely without them, from the difficulty of transportation.

The high price of building material put comfortable shelter beyond the reach of all, except a favoured few. Great numbers who had arrived too late to provide quarters in the mountains and lay in provisions, had bivouacked along the river and in the suburbs of the town. With no covering but their tents, or beds but their blankets, barely raised from the wet earth, clothing filthy and covered with vermin, their condition when sick was wretched in the extreme. The immigrants were exposed to all the hardships of a camp without the discipline of an army, and the comforts or conveniences which the foresight of a quartermaster provides, and the intelligence and care of a medical staff supply. Although they started from home with partial organisation, very few of them held together after touching the auriferous earth. Each man was thrown upon his own resources, ignorant of the dangers by which he was surrounded, and insufficiently provided to meet them. Disappointed in his prospects, and dejected in spirits, he fell an easy victim to the influence of a climate he had been led to believe the most salubrious in the world.

The sick were often deserted by their friends to shift for themselves, or were shamefully neglected to despair and die. The ranks of the medical profession were crowded with pretenders, and a feeling of distrust was general where all were strangers, and caused many to reject all medical aid. A few were provided for by private charity; but the greater number were suffered to perish uncared for, when they could no longer render an equivalent for services. A provisional government was organised; but, without resources, it could do but little more than furnish coffins. A debt of twelve thousand dollars was incurred for that one item alone. In April following, I counted close on to one thousand graves in the vicinity of Sacramento, over which the grass had not yet grown. The destitute were so numerous that the greatest exertions were required to furnish food necessary to support life, and large amounts were contributed by some of the more successful merchants. Nor were the unobtrusive and self-sacrificing efforts of many worthy members of our profession small. Drs Cragin, Deal, Morse, Riggs, Bay, Higgins, and others, will not be soon forgotten by those who were the subjects of their charity, though no pen took note of their deeds or gave their names to the world. Some of them involved themselves seriously in their sacrifices to shelter and provide for the sick poor.

Imperfect digestion and irritability of the bowels was an early, constant, and almost universal affection, and, from the habits of society, abundant facilities were afforded for observation. The evacuations were large and lienteric, often liquid, sometimes clay-coloured, but generally bilious. These often continued for many weeks, and disappeared under an improved regimen. Many continued for months without materially affecting their capability to attend to their usual avocation, and degenerated into a chronic diarrhœa. It was a rare thing to find a man whose bowels were not affected, in some degree, in the way that I have described. Even in those cases where digestion was well performed, the stools were bilious, and the rectum in an irritable state, notwithstanding mucous or sanguinolent discharges were uncommon. Intermittent fevers were still very prevalent, but not fatal. Remittents were common, but I thought not necessarily fatal, with proper comforts and care. Congestive forms of fever were rare; I saw but one fatal case. Typhoid fevers were very destructive. They were distinguished by a strong tendency to delirium, and coma set in early. The inundation occurred about the end of the first week in January, and put an end to the intermittents, for the most part, and from that time, the diarrhœas were less common. I am not aware of any new cases of diarrhœa occurring after the "great flood" in January. The mild cases generally recovered, while the more confirmed ran on to a fatal termination. This diarrhœa was so general during the fall and winter months, and degenerated so frequently into a chronic and fatal malady, that it has been popularly regarded as the disease of California. About the middle of December, a suitable two-story frame building, which had been contracted for by Dr J. F Morse and myself, was opened for the reception of the sick, and an opportunity was presented to make a more intimate acquaintance with disease.

From the 1st of January to 1st April, the number of admissions was sixty-four, immigrants from twenty-two different states. Of these the numbers that came by the way of Cape Horn and the plains were about equal. Most of them were sent to us by the city officers, and were in the most destitute and filthy condition. Twenty-five were admitted with chronic diarrhœa, and of whose history previous to their admission we had no other knowledge than from their own imperfect account. The average duration of the diarrhœa, in fatal cases, from the time of attack to their death, was four months. This diarrhœa, in those cases that were so severe as to call for medical treatment, was distinguished by copious liquid stools without pain, or but momentary griping, without acceleration of pulse, heat of skin, or depression of spirits. The tongue was moist, the appetite good; and the greatest complaint was from the inconvenience of the disorder. This condition wonld continue sometimes for weeks, resisting every treatment. Nor was it confined to the most destitute. Many were affected with it who were as well supplied with comfortable quarters and wholesome food as the state of the country would allow. Emaciation was generally the first effect which gave rise to serious apprehension on the part of the patient, and indicated the confirmed stage of its progress.

As the disease advanced, the tongue became glossy, red, and fissured, as in chronic gastritis; the stools grew dark-coloured, often mixed with purulent and fibrinous matter as from ulceration. Towards the close, tormina were troublesome, though tenesmus was present in no one of the twenty-five cases, except a short time before death; then in but few cases, and never severe. It was observed that the discharges were the most frequent during the first part of the night. Before death, emaciation and anemia became extreme, and the mind which, in most cases, had been cheerful and hopeful, at last gave way to the most childish imbecility.

By nothing was this affection more distinguished than by its fatality. Of the twenty-five cases of chronic diarrhœa, fourteen died before the 1st of April, and the most of those remaining are known to have died since. So fatal was this disease regarded, that a late writer in the New Orleans Medical and Surgical Journal was led into the extravagance of believing, from the reports of the old settlers and physicians then practising in Sacramento, that a disease resembling Asiatic cholera there existed. I have no doubt that the number of deaths in California, from chronic diarrhœa of the character described, was greater than from any other disease.

The treatment of this diarrhœa, when commenced early, was indicated by the enfeebled digestion. It was regarded as of the utmost importance to aid the recuperative powers by generous living. Stimulants were often used to advantage with those unaccustomed to their use, sometimes combined with astringents. A favourite combination with me was a tincture of black pepper and kino with brandy. Some cases which were attended by symptoms of irritation were benefited by opiates; but the indiscriminate use of opium I have no doubt was a great aggravation of the difficulty, and often ensured a fatal termination. When the disease became confirmed, stimulants were no longer serviceable; it was aggravated by vegetable acids and vegetables; though from the symptoms of scurvy in some cases, and the fear of it in others, it was thought necessary to use them in such forms as were the most easy of digestion. Opium in this stage served to check the frequency of evacuations, but only to increase their quantity. Acetate of lead, nitrate of silver, and sulphate of copper, in combination with opium, were tried, but so generally attended with nausea from the gastric sympathy, that, though I sometimes fancied that one or other of them were serviceable in particular cases, we finally abandoned the use of them altogether, relying chiefly upon diet.

Boiled milk, when it could be obtained, thickened with fine wheat flour, or either of them alone when the other could not be obtained, seemed to excite the discharges less than any other articles of food. But the appetite of our patients was so insatiable that no opportunity of devouring whatever food they could obtain was neglected. They were well provided with warm dry beds, and though the wards on the main floor were filled with water by the inundation, and afterwards so damp as to be unfit for use, our second floor was dry and well ventilated. They were constantly supplied with whatever we thought would contribute to their comfort or recovery, except milk, which was scarce. To quinine I did not give a satisfactory trial, though it has been spoken of in high terms by others when used early.

The following table shews the number of admissions from 25th December 1849, and the number remaining on the 1st of April following with the diseases for which they were admitted, the recoveries and deaths.

Disease.	Cases.	Cures.	Deaths.	Remaining.
Bronchitis,	1	1		
Chronic diarrhœa,	25	5	15	5
Insanity,	3	Dismissed.		
Remittent fever,	15	12	2	1
Typhoid fever,*	7	...	7	
Chronic Rheum.,	2	...	1	1
Scurvy,	9	7	...	2
Caries of spine,	1	1		
Pneumonia,	2	2		
Albuminuria,	1	...	...	1
Casualties,	4	3	1	
Delirium tremens,	1	1		
Starvation,	1	1		
Unknown,	5	...	4	1
Ascites,	2	...	2	
Typhus,	2	2		
Total,	81	35	32	11

Pulmonary complaints were rare at Sacramento. I do not remember to have met with a single case of tubercular consumption. This may be accounted for, in part, by the character of the immigrants; but there were not a few, who from a supposed tubercular diathesis, sought a change of climate in California more favourable to them, and have enjoyed better heath than in the Atlantic States; while many others, who at home were in robust health, here suffered emaciation, and under the most favourable influences could not recover their loss, with no appreciable cause but the atmospheric agencies.

Persons who had been spending the summer in the valley were very liable to icterus when they spent a few days at San Francisco, and friends meeting below were often surprised to see each other with such jaundiced faces.

REFLECTIONS.

The fatal effects of dysentery in armies is but too well known. The records of military surgery give it a prominent place. Says Moseley: "The page of military history weeps less for the slain in battle than for those who have fallen victims to this calamity."

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* Received in a typhoid state; the original form of the fever uncertain.

The careful investigator, however, will distinguish a wide difference in the character of the epidemic either at different times or localities. Dysentery, in the works of British army surgeons in Europe, the East and West Indies, appears as a uniform affection distinguished by the following characteristics: "Besides some feverish symptoms, a disorder at the stomach, and wind in the bowels, small but frequent stools of a slimy and frothy matter, tenesmus, and gripes. Blood mixed with the fæces, is a common but not a constant symptom. These may be called the pathognomonic symptoms, and as such may distinguish this illness from a diarrhœa, an hæmorrhoidal, and all other fluxes."* He observes that all the epidemic dysenteries which he had seen in the army were of the same nature, and cured by the same remedies. Johnson, Annesley, Moseley, and others, are equally explicit, and though they speak of diarrhœa, it is passed lightly over, as being merely a precursor of the more fatal disease. Annesley goes so far as to say that death from these affections is generally caused by perforation of the intestine, and discharge of the contents into the peritoneal cavity.† No one can believe that if diarrhœa, as it occurred in California, had prevailed to any extent as a fatal affection, it could have escaped the notice of such observers.

The sudden fatality of some epidemic camp dysenteries, deserves a passing notice. Immense armies have been half destroyed in the space of a month after their taking the field.‡

The British army in Egypt suffered severely from diarrhœa and dysentery.§ The troops were represented as having been a long time at sea on the use of salt provisions, and afterward lived on fresh mutton and buffalo meat. They were also destitute of vegetables and sometimes of salt, exposed to sudden transitions of cold, and inattentive to cleanliness. These facts, taken in connection with the physical geography of the country, would leave us to look for a similar form of disease; but the diarrhœa, as described by Dewar, was a mild form degenerating into dysentery, and differing only as stages of the same affection. The dysentery was distinguished by the same symptoms already referred to in Pringle, and nausea was a constant attendant, and adventitious membranes common. Macculloch says, "I should remark, however, that whatever well-known and essential distinctions there may be between dysentery and diarrhœa, there are instances in practice where in an autumnal disease of this general character, arising in persons exposed to malaria, the term diarrhœa is the most applicable.*

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* Rush's Pringle, p. 194. Sixth edition. London, 1768, p 218.

† Annesley, vol. ii., p. 343.

‡ See Moseley, p. 218.

§ Dewar, Observations on Diarrhœa and Dysentery. London, 1805.

The great tendency to diarrhœa in convalescence from intermittent and remittent fevers is well known. It is described by Maillot as a serous or sero-sanguinolent diarrhœa, without pain and without fever. He had thirty cases admitted into his wards at the military hospital at Algiers, during the last months of the autumn and first months of winter. Their character was very uniform. Some of them occurred at an advanced stage of convalescence, others as an extension of the primary affection.† Broussais has furnished some cases occurring at Udina, in the north of Italy, under like circumstances, that appear identical in character with the chronic diarrhœa described by Maillot. They were without colic, tenesmus, and fever; they were also attended with great appetite, and the food was almost immediately rendered at the anus undigested. After continuing above a month, with two or three evacuations in twenty-four hours, they died in the last stage of marasmus. He asks, whence does this difference arise, that in some localities dysenteries preponderate, in others, diarrhœas?‡ Many cases of Californian diarrhœa were preceded by intermittents; and although many do not appear to have suffered in that way, still it must be admitted that all were much exposed to the intermittent malaria. Whether preceded by fever or not, the diarrhœa was distinguished by the same symptoms, occurred at the same time and place, and was equally fatal.

Broussais observed also "in Italy a great number of persons attacked with diarrhœa, without any other appreciable cause than the influence of climate and irritating indigestible articles, and unattended with any inconvenience except from colicky pains preceding each dejection from the bowels. These persons were not prevented by debility, and by the painful confinement resulting from the frequency of these stools, from being able to attend to their business until after several weeks. So long as they did not change their accustomed mode of living, the diarrhœa did not cease; it might be prolonged for six months in this way, but it gradually exhausted the patients."§

In respect to its fatality, he says, that "there is a period beyond which the best directed treatment can no longer prevent the successive degeneration of all the functions. Diarrhœas of more than three months' continuance have all been fatal when I have attempted to cure them." Of Maillot's thirty cases, fourteen died and one remained. Had these facts been better understood at San Francisco, there would have been less censure bestowed by the public press on the unwearied, but in a great measure unsuccessful, efforts made by the hospital physicians to control the ravages of that disease, and reduce the fearful rate of mortality.

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* Macculloch on Fevers. Phila. ed., p. 113.

† Maillot, Traite des Fevres intermittent, p. 246.

‡ Broussais. Phila. ed., 1831, p. 122.

§ Ib., p. 158.

Is this specific difference of enteric disease endemic? Time and a more careful attention to data will no doubt determine. Our army in Mexico suffered severely from this form of the disease, and I am informed it is still following the remnant of the New York regiment. One case that occurred in my private practice in California, was contracted on the route through Mexico, and the patient died at Sacramento nearly a year afterwards; I could distinguish no difference between this and those contracted at California. In the *N. Y. Journal of Medicine* for Nov. 1848, is a table by Dr Newton, U.S.A., shewing the number of deaths in the city of Mexico for the year 1844. By that table it appears that the number of deaths in that city from diarrhœa exceeded that from any other disease, not excepting dysentery, being as 1 to 9·9 of all other diseases, while in the city of New York, by the last Inspector's report, it is as 1 to 35·8. The number of deaths from intermittent, although stated to be not a fatal disease, was in Mexico as 1 to 136 of all others; in New York as 1 to 1415; there being but twelve deaths from that disease, and these probably introduced or occurring only in the thinly populated part of the town,—the great mass of the population not being subject to the influence of the malaria.

Pringle observed that as the autumn grew cool the fevers abated, and changed into an irregular and bad intermittent, alternating with dysentery, and inferred the existence of a like cause; and though many have since advocated the contagious nature of chronic camp dysentery, the theory of a malarious origin has been gaining strength until there are few intelligent physicians who do not acknowledge that disease to be the product of that influence.

If the diarrhœa of which I have been speaking has a similar origin with dysentery, is it from a distinct variety of malaria, or is it modified by collateral influences as yet inappreciable? There is no doubt that hardship and the want of the customary comforts of home, more especially insufficient or unsuitable food, did much to predispose the system to the effects of malaria, but of themselves do not appear to be sufficient to excite so incurable a disorder.*

Malaria seems to be the only necessary element in the generation of this chronic diarrhœa; and if the Cryptogamous theory, so ably advocated by Professor Mitchel, could be sustained, and the *modus operandi* be that of irritation on the intestinal mucous membrane directly, would it not furnish a more simple and satisfactory explanation than has yet been offered, for all the phenomena attending this disease?†

If what I have stated as fact, be such, is this subject not worthy of a careful investigation? Every steamer that returns freighted with adventurers from that distant land, brings home the victims of this fatal disorder only to die; while hundreds are lingering there, unconscious of the slow but inevitable fate that is leading them to their obscure graves in the mountain cañons;‡ and thousands more, who will frequent California's rock-bound shores, and people her solitudes, will fall before this malady, that may still be said to "walk in darkness."

NOTE.—Since the foregoing was presented for publication, the writer's attention has been called to a paper, "*On the Diarrhœa of the South,*" by J. P. Evans, M.D., of Tazewell, Tennessee, in the May number of the *Charleston Medical Journal and Review.* His views seem to have been suggested by experience in the army of Mexico; and any one who has been sufficiently interested with the preceding pages to follow me as far as this note, cannot fail to be interested by his remarks. However I may fail to be convinced of the truth of all his pathological opinions, I can but congratulate myself in having been permitted to peruse this paper, and cite the testimony of the author to confirm the conviction that has been gaining strength with me since my absence from California, that the disease which I have described is endemic; of malarious origin, in common with intermittent and remittent fevers; and is a "peculiar affection dependent upon primary pathological condition, different from those derangements which gave rise to diarrhœa in general." In the treatment, he recommends quinine, brandy, and opium. In these forms which were most liable to become chronic, he observed that stimulants were very serviceable in recent cases. He says, "I have known a hearty draught of red pepper tea, and whisky or brandy, to allay the intestinal disturbance completely and permanently; but stimulants combined with narcotics, present the most efficient medicaments in the treatment of such cases." His patients were nearly all cured; though, from his own statement, and the bills of mortality in the army while in Mexico, and since their return from that country, from diarrhœa contracted there, it is but a fair inference that he lost sight of his patients too soon. Instances fell under my notice where the patient was repeatedly discharged as cured, and died long after from the same disease, —a fact which should have been noticed in its proper place.

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* See Broussais, p. 143; Annesley, vol. ii., p. 388; Bampfield on Scorbutic Dysentery; and Johnston on Tropical Climates.

† Edinburgh Medical and Surgical Journal, vol. lxxiii., p. 473, April 1850.

‡ Cañons is a Spanish term. Here it means gulleys or deep valleys.

ART. III.—*On the Climate and Diseases of California.* By JAMES BLAKE, M.D., F.R.S. (American Journal of Medical Sciences. July 1852, p. 53, No. 47.)

The investigation of the modifications produced on diseased action by the influence of external circumstances opens an interesting field to the researches of the medical philosopher; for it presents an example of an experiment made by nature on a vast scale, in which some of the most important elements modifying disease are so completely changed, that we have an opportunity of observing the phenomena they present, under circumstances which would never have been produced by artificial means. This consideration invests the subject of the investigation of disease in this country with a degree of interest, which renders it worthy the attention of those who take a pleasure in elucidating its complicated phenomena; and whilst fully realising the importance of such an investigation, I have to regret that the data which a limited practice has enabled me to collect, can throw but little light on the subject. The facts, however, that have been observed, in relation to the climate of our winter months, will probably be interesting to many of our readers. The data recorded have been only those which are likely to influence the progress and character of disease, and, as it will save space, I will present them in a tabular form.

From the following table it will appear that the most important feature in the climate of this country, during the winter months, is its equability.

	Nov.	Dec.	Jan.	Feb. 1 to 16.	March 8 to 31.	April.
Mean temperature	51°	44·6°	47·7°	49·2°	52·2°	60·7°
Highest temperature	73	68	70	71	79	93
Lowest temperature	31	26	31	28	32	39
Mean of maxima	66·2	57	60	64·4	66·6	77·5
Mean of minima	42·6	36	37·2	37·8	44	49·3
Mean temp. of north winds	48	41	46	51	51	63
Mean temp. of south winds	55	50	50	49·5	50·7	58
Mean minima of north winds	34	32	38	39·5	37	49·2
Mean minima of south winds	48	43	39	38·5	42	48·5
No. of days of north wind	15	7	10	6	5	5
No. of days of south wind	12	8	4	4	12	12
No. of calm days	13	16	17	6	7	14
No. of clear days	16	20	19	13	9	13
No. of cloudy days	7	6	6	3	8	12
No. of rainy days	7	5	3		7	4
	inches.	inches.	inches.		inches.	inches.
Amount of rain	2·7	2·5	0·7		2·5	1·8

In no instance has the thermometer fallen more than 21° below the mean temperature in any one month, and the difference between the mean temperature taken at the coldest part of the day, and the lowest temperature observed during any month does not exceed 11°. This fact presents a striking contrast with the variations of temperature observed in the Atlantic States, where it is not at all uncommon to find the thermometer descending 30° or 40° below the mean temperature, even of the minima. In Rome, where the average temperature of the winter months is about the same as here, the thermometer sometimes descends as low as 23°, whilst here it has not been noted below 26°; and there can be no doubt that the winter of 1850-51 was a cold one, on account of the absence of strong southerly winds and rain, which are always accompanied, during the winter months, by a higher temperature. From all I could learn from those who had been in the country for many years, the temperature of 26° was a cold they had never before experienced. This season, 30° is the lowest point that has been reached; in fact, the temperature of the winter months is most congenial, being generally about 36° at sunrise, rising to about 60° during the day, and again falling towards evening.

The direction of winds in this part of the valley is, as a general rule, N. and S., or varying but very slightly from these points. I have never observed an easterly wind; and this absence of easterly winds is a fortunate circumstance as regards the climate of the valley, as a strong wind from that direction, blowing across the snowy peaks of the Sierra, could not fail to bring with it unpleasant indications of the country over which it had travelled. The influence of the winds on the temperature varies according to the season of the year. In the winter months, the north winds are the colder, and the south winds, which, when strong, are always accompanied by rain, are warm. Towards the spring, the north winds become warmer, on account of the sky being generally clear when these winds are blowing, and the sun having more power raises the temperature. It will be seen, however, that the mean minima of the north winds, or the lowest temperature observed on those days on which the wind is from the north, is still below the minima of south winds until the month of April, when the summer climate decidedly shews itself, in which the north winds are always the minima. As regards the force of the winds, it is generally but slight: it only blew hard from the south twice during the whole of last winter, and the north winds are generally very light. The atmosphere is usually calm, or only agitated by slight local currents.

It will be seen, also, by the table, that the sky is, as a general thing, clear, and this is so during the only season of the year in which a cloud is ever to be seen in the heavens. The quantity of rain that fell during the winter of 1850-51 was about eleven inches. There can be no doubt but this was much below the average, and probably not more than a fifth of what fell in 1849-50. During the whole of the winter months, the air is decidedly moist, the dews are extremely heavy at night, the air is frequently loaded with fogs in the morning, and it is rare to see a difference of more than six or eight degrees between the wet and dry bulb thermometers.

On account of being engaged in geologising in the mountains during the summer, I have not any extensive series of observations shewing the nature of the climate during this season. The principal characteristics of our summer climate are, a high temperature during the day, cool nights, and a dry state of the atmosphere. From some few observations made at Sacramento, during the months of July and August, the average temperature at sunrise was 63°, at 3 P.M. 94°. The highest temperature noted was 98°, although I was not in the valley during the hottest days. On one occasion, last summer, the thermometer rose to 106° in the shade; this was in the mountains, and it is probable that the thermometer was almost as high in the valley. During the summer months, a southerly wind generally prevails, rising about eight or nine o'clock in the morning, and continuing until sunset, or even during the whole of the twenty-four hours. This tends to moderate the temperature, tempered, as it undoubtedly is, by the cold sea-breeze that blows so constantly on the coast. The hottest days are those on which the wind is from the north, loaded as it is with the heated air of the valley between the Sierra and coast range. But the most important element, as regards the summer climate, is its hygrometric condition. From observations made in July and August, the mean quantity of humidity contained in the atmosphere was not, during the hottest part of the day, more than 40 per cent. of that required for its saturation, and I have not seen it rise above 74 per cent. even at sunrise. In September and October, the temperature becomes lower, and at the same time a considerable change takes place in the condition of the atmosphere. The sky still remains unclouded and the weather dry; but the winds, which are rarely absent during the months of May, June, July, and August, are now seldom felt, and the air becomes more moist. The mean of some observations made in September gives 80° as the mean maximum, and 60° as the mean minimum. The atmosphere, at the maximum, contained 66 per cent. of moisture, and in both it had increased to 73 per cent., although no rain had fallen.

I will close my observations on the climate of this part of California by a few general remarks. The data, as far as they go, shew that the temperature of the winter months is about the same as that of Rome, without being subject to such variations as are experienced in that climate. Another advantage, also, which it possesses over the climate of Italy is, that here the rains are always accompanied by warm winds, whilst at Rome the atmosphere is frequently exceedingly cold and chilly in rainy weather. As before stated, I believe that the temperature of the winter 1850-51 was below the average. The temperature has certainly been milder during the present winter; the thermometer has not been lower than 30°, and at that point but on one occasion. During the summer months, the weather is most agreeable; for, notwithstanding the high temperature, the constant breeze and the dryness of the atmosphere prevent the system from being oppressed by the heat; whilst the wet bulb thermometer ranges from 60° to 70°. Even during the hottest part of the day, there is no danger of the human frame, with its large evaporating surface, being injuriously affected by heat. The only period at which the heat is felt oppressive is in the months of September and October; a temperature of 80°

will produce more inconvenience here than a temperature of 96° in the month of July, on account of the difference in the hygrometric state of the atmosphere.

These observations regarding the climate of this country will apply only to the valleys of the Sacramento and St Joaquim, or the district comprised between the Sierra and coast range; and as a short notice of the topography of the valley may be useful, I will, before commencing any remarks on the diseases, give its general outlines. The valley extends a distance of about five hundred miles north and south, varying in breadth from forty to eighty miles, from the lower hills of the Sierra to the coast range of mountains, the west of Sierra being from sixty to one hundred miles farther to the east. Through the valley run the Sacramento, coming from the north, and the San Joaquim from the south. These two rivers drain the waters from the western slope of the Sierra, and from the eastern slope of the coast range. They find their way to the ocean through a considerable break in the coast range. The rivers are bounded on each side by extensive flats subject to annual overflow, and forming an immense extent of marsh land; these flats are intersected, in all directions, by extensive sloughs and lakes, which frequently have no communication with the river except at the time of high water. In fact, as a general thing, the immediate banks of the river form the highest part of the country, so that, when the waters fall after an overflow, a very large extent of country is left undrained, from which the stagnant waters are gradually dried up by evaporation under the rays of a powerful sun. At the time of writing this, the water is covering an extent of country ten or twelve miles in breadth, and in some places much broader. The soil of the valley is a fine alluvium, in some parts sandy, in others composed of tenacious clay. These superficial layers repose on alluvial strata, which, being generally gravelly, allow, I believe, the water to drain off to some extent, although there is always a very large surface from which it disappears, but by the slow process of evaporation. The short time during which I have been practising in the Sacramento would not justify me in going into any details, or attempting any generalizations on the diseases of this climate. I propose merely to make a few observations on some of the cases that came under my care, and I have appended a list of the cases admitted into the State hospital in this city, although that institution receives by far the greater part of its inmates from other portions of the State.

During the months of October and November 1850, the principal diseases were dysentery and diarrhœa: 85 per cent. of the cases I attended during these months, were of that nature. The dysentery was of a very untractable character, wearing the patient down by frequent bloody discharges, and even when checked being constantly reproduced. It was very fatal. In the absence of any positive data, I believe about 30 per cent. of those attacked died. The great mortality was not so much owing to the virulence of the disease itself as to the peculiarly unfavourable circumstances in which those attacked by it were placed. By far the larger part of the sick, probably 90 per cent., consisted of emigrants who had just crossed the plains, having accomplished a journey which, at all times fatiguing, was in the year 1850 attended with the most trying circumstances. By far the greater number of emigrants who arrived here in the autumn had not only been broken down by fatigue, but had been forced to subsist on a short allowance of food, and that frequently of a bad quality. Under these circumstances, it is not surprising that the first opportunity that was afforded them for satisfying their appetite should have been made an occasion for overloading the stomach, and thus producing diarrhœa. The sudden change from the dry, bracing atmosphere of the interior of the continent and the mountains, to the comparatively heavy, warm air of the valley, exerted a most depressing effect on the system. On this point I can myself testify; for, although without any apparent sickness, yet for the first three weeks after my arrival in the country, from crossing the plains, I was so prostrated that I could lie on my bed during the whole day, without the slightest energy, either physical or mental, and many of my friends were affected in the same way. Disease, I believe, is often brought on by men attempting to work while in this state of prostration. The diet, also, was a fruitful source of disease, not only as regards its quantity, but even its quality. The only fresh meat to be had was beef, and this was generally fried, or rather boiled in rancid lard. The flour was to a great extent damaged, and the butter and salt meat were all more or less rancid. I state these facts to shew that, although diarrhœa and dysentery prevailed here to so great an extent, and so fatally, during the fall of 1850, the causes of its ravages are to be looked for principally in the unfavourable circumstances in which the population was placed, rather than to any malarious influence. But not only were these circumstances fruitful sources of disease, but they presented an almost insurmountable obstacle to recovery, even when the more violent symptoms had been subdued. There was no such thing known here as that valuable hygienic remedy called nursing. From the toast water of the sick room your patient had to go back to the beef, salmon, and rancid grease of the boarding-house. There were none of those light and valuable edibles which gradually lead the invalid by a safe path back to his ordinary diet. Relapse after relapse occurred, and it wanted but little assistance from the diarrhœic influence which generally precedes the appearance of cholera, to produce a state of gastro-enteric disease amongst four-fifths of the newly-arrived population.

In this state of the general health, the cholera made its appearance about the beginning of November, and never did it fall on a population so prepared to yield to its ravages. Although it is impossible to obtain any accurate data as to the number of its victims, yet I am confident that, during the few days in which it was at its height, not less than one per cent. of the population was daily carried off by it. There was nothing peculiar in the disease as it occurred in this country; its apparent virulence can be accounted for by the previous state of those attacked. The table appended, shewing the return from the State hospital in this city, proves that, even during the summer and autumn months, there is very little tendency to gastro-enteric disease, at least when the exciting causes above alluded to are absent.

The diseases more particularly characteristic of the winter months are typhus fever, rheumatism, erysipelas, and pneumonia. The cases of typhus that came under my care were of a mild form, although generally lasting from fourteen to twenty-one days. In none of the cases which I treated did any unfavourable symptoms present themselves. The treatment was purely expectant, with the exception of quinine or bark, and stimulants towards the latter period of the disease. In two instances, the rose-coloured spots were noticed on the sixth day of the disease. From the returns of the State hospital, it would appear that this disease was prevalent during the autumnal months of last year, and the mortality was 39·7 per cent., a decidedly large mortality. Only three cases were admitted from the city, the others being brought in from the country.

The cases of pneumonia shewed themselves soon after the first rains, and could generally be traced to exposure to wet and cold. They were very amenable to treatment. Only three cases came under my care, and in every case the patient made a rapid recovery, the time of treatment averaging nine days.

Rheumatism was one of the most common diseases during the winter months of 1850 and 1851, attacking principally those who had been previously affected by diarrhœa and dysentery; and in these it generally presented itself with very troublesome, and in fact alarming, symptoms. The disease was generally of a low type, and in every case that came under my care was accompanied by inflammation in the muscles, or at least in the aponeuroses; this inflammation was the principal and most distressing symptom. In two of these cases suppuration took place; in one subject, in the calf of the leg, under the gluteus maximus, and under the trapezius; and, in the other, in the calf of the leg. In three other cases that I treated, these swellings were present in the calf of the leg; and although I was led strongly to suspect that pus had formed, yet they recovered without any abscess being opened. Pericarditis existed in three cases, but, notwithstanding the unfavourable character of the general disease, this complication did not present itself in a severe form. One of the cases terminated fatally,—that in which matter had formed so extensively. I believe the case would have been saved had it remained under my care; I was, however, superseded by an herb doctor, whose first care was to bind up the abscesses as tightly as possible, to prevent the discharge from weakening the patient. The treatment adopted was the administration of colchicum and quinine, and sometimes sulphate of iron; recovery was slow; the average time of treatment being forty-one days. I had an opportunity of seeing some other cases, in which the same symptoms shewed themselves. Rheumatic affections are not so common amongst the mining population as we might be led to expect from the exposure to which they are subject, working, as too many of them do, for hours at a time in the water, and also frequently exposed to rain. But very few cases of this disease have been admitted into the hospital during the past year; and this is probably owing to the absence of diarrhœa and dysentery, which seem to exert a peculiar influence in predisposing the system to rheumatism.

Erysipelas is a disease which has been extremely fatal in the country during the last twelve months; but it has existed to a greater extent in the mountains than in the valley. During the course of last winter, five cases came under my care; in all of these the head was the only part involved, with the exception of one, in which the inflammation spread to the shoulder and chest; three of these cases occurred in the same house, and the first case that shewed itself was in a man who had been living in another house in the country where there was a case of the disease. The most striking peculiarities of these cases were the inflammation of the mucous membranes communicating with the natural openings, and the absence of that amount of general and cerebral disturbance which so often renders erysipelas of the head a formidable disease. In every case, the mucous membrane of the mouth and fauces was inflamed; in four cases, there were ulceration and discharge of fetid pus from the nostrils; in three cases, the palpebral conjunctiva was severely affected; and, in four of the cases, there was discharge of pus from the ears. And yet, notwithstanding these symptoms, the cases in which they presented themselves were attended with as little cerebral and general disturbance as any I have ever seen, in which the head was involved. The fever never rose high, nor was depletion required in any case. A dose of blue pill and castor oil, and then quinine, combined with small doses of blue pill and rhubarb, was the treatment usually adopted. The only case that terminated fatally was a man of intemperate habits, who died from a complication of the disease with pneumonia. The average duration of the treatment was nine days. This disease is of frequent occurrence in the mountains, and I have seen many cases of it during the summer. The localities where it mostly prevails are the deep valleys in the mountain districts, which are shut in on all sides by high hills, where the extremes of temperature are great, and the air has a tendency to become stagnant. In some of these places it has proved extremely fatal, the mortality being more than fifty per cent. This large mortality is to be accounted for partly by the intemperate habits of many of the subjects attacked by it, but also in a great measure, I believe, from the too general use of calomel, and from not giving quinine and tonics at an early period. In two of the cases that came under my care, granular conjunctivitis remained as a sequela of the disease; and I have seen some very severe cases of this affection that have been produced by it.

Diseases of the air-passages are extremely rare. A mild form of catarrh shewed itself apparently in an epidemic in February 1851, and the same affection is occasionally met with during the winter months; but I have not yet seen a case of acute bronchitis, and the experience of many of my medical friends in the city is to the same effect. This is probably owing to the absence of cold dry winds during the winter months. Very few cases of phthisis are met with, considering the circumstances in which so large a portion of the population is placed, and which in other climates could not fail to develop the latent germs of this disease which exist in so many individuals. Speaking from my own limited experience, it is more amenable to treatment here than in any other country in which I have practised. Two cases of the disease in an early stage have come under my care, occurring in individuals who had a strong constitutional tendency to it; in each case, hæmoptysis to some extent had occurred, and that partial solidification of the lung, indicated by the prolonged rough expiratory murmur evidently existed. One of these cases has apparently quite recovered, the individual having enjoyed good health for some months. The other case is improving, having gained six pounds in weight during the last month; cod-liver oil, with quinine and iodide of iron, has been the remedy used. It is an interesting fact, that in neither of these cases was any marked symptom of bronchial irritation present, and I believe the exciting cause of the disease had, in each case, been improper diet, or living too long on salt meat, which probably was rancid. A better diet was ordered, and strictly adhered to; and it is the advantages derived from this, and also from the complete absence of any source of bronchial irritation, that have rendered the disease so amenable to treatment. Many cases prove fatal, as well here as elsewhere; but I believe that the climate of this country presents advantages such as are not to be found in any other part of the civilised world, for the treatment of phthisis in its earlier stages.

The diseases arising from malaria might be expected to be very prevalent in the valley of the Sacramento, containing, as it does, thousands of acres of lands subject to annual overflow, and from which there is no escape for the water except by the slow process of evaporation under the rays of an intensely hot sun. Yet, notwithstanding this, cases of intermittent and remittent fever are comparatively rare in this part of the valley. A few mild cases of intermittent fever occurred in the neighbourhood of the city last spring, but they appeared to be confined to low localities which were situated in the immediate vicinity of stagnant water. I have heard of but two cases of congestive fever, and both of them on low bottoms near the American River, about three miles from the city. As a general rule, the city is exempt from any of the more violent forms of malarious disease; a few mild cases of intermittent and remittent fevers occur, but they are comparatively rare. The whole number of cases admitted into the hospital in this city, which originated in Sacramento, was twelve during the last summer and autumn. But, although the influence of malaria does not shew itself by producing any of the more marked forms of disease by which its presence is usually manifested, yet we have constant indications of its existence, by the character it impresses on almost every form of disease occurring in this locality. It would appear that, when in the usual state of health, there are very few individuals in the community who are susceptible to its influence, but when the resisting powers become weakened by any form of disease, the malarious influence then makes itself felt, modifying to a greater or less extent a very large amount of the cases of other diseases. This influence it is often difficult to detect; it is not manifested by any marked symptoms of intermission or even remission, but can only be suspected from the apparent obstinacy of the disease, and from the effect of treatment. Dysentery, diarrhœa, erysipelas, rheumatism, chronic gastritis, and even pyrosis, cases in which the remedy might appear to be strongly counter indicated, were benefited by the use of quinine, and, in fact, would not yield without it. This modified existence of malaria, in a locality where the elements from which it is generated surround us on every side, is probably to be accounted for by the dryness of the atmosphere during the hottest months in the year; by the moderate temperature that generally prevails when the amount of humidity in the atmosphere would favour its propagation or diffusion; by the prevalence of constant breezes during the summer months; and by the coolness of the nights, which conduces to refreshing and invigorating sleep.

As regards the number of cases of bilious remittent reported to have been admitted into the hospital, I believe they were for the most part cases of the mildest form of remittent fever. I understand from many medical gentlemen who practised here during the summer, that they did not see a single case of bilious remittent, and the short time the patients remained in the hospital (on an average eight days)* would indicate a very mild form of remittent fever. From the most accurate information I could acquire (for no records are kept), the greater number of the cases came in from the upper part of the valley, more particularly above Marysville. Scrofulous diseases are rarely met with, and I believe the climate will be found to offer the most favourable conditions for the prevention and cure of this class of diseases.

Diseases of the skin are rare; eczema is the only one I have met with. Onychia is common among the mining population, and slight wounds on the hands are very apt to prove troublesome; this is probably owing to the nature of the diet; for even when wholesome articles of food can be obtained, they are generally spoiled by boiling them in rancid grease. The quantity of rancid grease and salt meat eaten in this country, with apparent impunity, offers but a poor confirmation of the views of Liebig, of the injurious effects of partially decomposed substances on the animal economy.

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* In drawing up this average, three cases were excepted, in which the patients remained in the hospital thirty-seven, fifty, and sixty-four days, and in which there was undoubtedly some complication.

Diseases of the nervous system are not very common, although I believe forming a larger proportion in comparison to the whole amount of disease than in the Atlantic States. A very large number of cases of insanity have occurred, thirty-eight insane patients having been received into the hospital from May to December. This prevalence of insanity is undoubtedly owing to moral more than to physical causes, although the extreme dryness of the atmosphere during the summer undoubtedly renders the nervous system peculiarly excitable. There is one fact with which I have been struck whilst travelling in the mountains during the last summer,—viz., the rare occurrence of *coup de soleil.* It might be supposed that the miners would be peculiarly liable to this disease, exposed as they are for hours to the burning rays of the sun, and frequently with their feet in ice-cold water; yet I have never seen or even heard of an instance of sun-stroke, although the rays of the sun are certainly more powerful here than in most of the Atlantic States.

The climate in the mining regions very much resembles that of the valley, and it is necessary to ascend some distance in the mountains before any marked difference is noted, even in temperature. I have no correct data on this point; but the state of vegetation would indicate that there cannot be much difference in the temperature. When I left Placerville (about sixty miles south of Sacramento), three weeks since, or in the middle of February, the young shoots of the buckeye were three inches long. The masoneeta (a species of Erica) was in full bloom. The humming-birds were building their nests, and I believe vegetation was quite as forward, and perhaps more forward, than in the valley. This was at an elevation of two or three thousand feet above the valley.

The mining regions are generally extremely healthy, with the exception of those localities which are situated in deep valleys, surrounded on every side by high hills. Here the extremes of temperature are very great. As a general thing, the nights in summer are calm, and the great radiation that takes place under a perfectly clear sky rapidly cools the layer of air in direct contact with the ground. This cold air can be felt running down every gulley and ravine leading into these deep hollows, and if there is not any large opening by which it can drain off, it accumulates and fills the valley with a body of dense, stagnant air, the temperature of which descends very low before morning. In such places typhus fever and erysipelas were very prevalent during last summer, and were also very fatal. With this exception, the mountain regions are very healthy; and it is a striking fact, in confirmation of this, that exposure can be borne here to almost any extent with impunity. During ten months in the year, the shelter of a tree is all that is required. When travelling in the mountains, I always slept in the open air except when it rained, and I never experienced the slightest inconvenience from so doing; nor have I ever seen any instance in which bad results followed, except in wet weather. This is very different from what we are taught to believe as regards exposure to the night air in the Atlantic States, with how much reason I cannot pretend to say, never having there tried the experiment. The general health of the community is, I believe, of a higher character than in any other part of the Union; the complexion here generally assumes that ruddy tint which is considered, and I think justly, as the most unequivocal sign of a high state of health in individuals of the Anglo-Saxon race. A residence of a few months in the country is almost always accompanied by a marked increase in weight, and there can be no doubt but that the climate is conducive to fertility in the female. Although I may expose myself to the charge of enthusiasm as regards the climate of this country, yet I am but expressing my candid opinion when I state, that I believe California will be found more conducive to the highest physical and intellectual development of the Anglo-Saxon race, than any other part of the globe. There is not a day in the year in which the powers of the mind or of the body are enervated by heat or numbed by cold. And when the agricultural resources of the country shall become developed, and the swamp lands reclaimed and brought under cultivation, I believe that every external influence, detrimental to the preservation of health, will be reduced to a minimum.

Table shewing the number of the principal Diseases admitted into the State Hospital, Sacramento City, with their average mortality and duration of treatment.

	June.	July.	August.	September.	October.	November.	December.	Total.	No. of deaths Per cent.	Average time of treatment.	Number of cases in Sacramento.
Intermittent fever,	...	6	6	11	12	4	6	45	1·2	7 dys	5
Bilious remittent fever,	4	6	21	21	38	18	8	116	4·3	8	6
Congestive fever, .	...	2	...	2	...	...	...	4	50·0	6	1
Typhus fever, . .	...	...	1	4	12	5	11	33	39·7	13	3
Continued fever, .	3	2	2	...	1	2	...	10	...	9	1
Erysipelas, . . .	...	...	1	4	3	1	2	11	18·1	7	
Diarrhœa, ...	3	1	...	1	1	3	...	9			
Chronic diarrhœa, .	4	3	2	3	5	3	1	21			
Dysentery, . . .	2	1	1	...	1	2	6	13	56		
Acute bronchitis, .	...	...	...	...	...	3	...	3	54		
Chronic bronchitis,	1	...	2	...	1	...	...	4			
Pneumonia, . . .	...	...	...	...	...	...	5	5	100		
Pleurisy, ...	1	...	...	...	...	1	...	2			
Acute rheumatism,	9	3	2	...	...	...	...	14			
Chronic rheumatism,	3	2	5	7	4	...	2	23			

ART. IV.—*On the Dengue or Break-bone Fever, as it appeared at Savannah in the Summer and Autumn of* 1850. By RICHARD D. ARNOLD, M.D., Savannah, Georgia. (The Charleston Medical Journal and Review, vol. vi., May 1851, No. 3, p. 323.)

The winter of 1849–50 exhibited to the practitioners of our city, as an epidemic, a disease which they had met before only in isolated cases; I allude to erysipelas, which commencing late in the fall of 1849, prevailed during the whole winter, and until the end of May, to such an extent as fairly to entitle it to be considered an epidemic. I mention this fact, not to give any history of it, but to shew that, taken in connection with the general spread and long continuance of measles in 1847 and 1848, the existence, to a considerable degree of scarlatina in 1849, and the subsequent appearance of the Dengue in 1850, there has been for the last few years what may be termed a decidedly epidemic exanthematous constitution of the atmosphere.

I use the phrase constitution of the atmosphere, for want of a better or more definite one; for, with all the resources of modern art, the ultimate cause of epidemic diseases has eluded research, and we are obliged to go into the domain of conjecture. The only appreciable modifier of disease is the temperature of the atmosphere, as we know changes in it are often the exciting causes of catarrhs, rheumatism, &c.; and temperature is itself modified by locality and latitude. In a few words, although a well-digested meteorological table may record all the variations of temperature, it cannot at any time afford a solution as to the ultimate cause of epidemics.

After all our inquiries and conjectures, we are compelled to content ourselves with repeating the old adage, "*Latet causa; vis est notissima.*"

During the spring of 1850, a few cases of scarlatina occurred in my practice; but although they were very severe ones, I could not say that they were numerous enough to justify me in stating that this disease was epidemic.

I was absent from the city during the month of May. June was remarkable for its pleasant temperature. Refreshing showers fell, and, although, as a general rule, this month is hot and dry, it was not so in 1850. There was very little sickness of any kind during this month, except a few cases of bowel affections. July, on the contrary, was intensely hot and very dry. The pleasant weather of June spoiled us for the heat of July, and every body seemed really to suffer from the oppressive heat. As a general rule, when the weather is dry as well as hot in the early summer months, there is very little sickness amongst us. Last July was no exception to this rule. About the end of July, the report reached us that the break-bone, or Dengue fever, was prevailing very extensively in Charleston.

About the beginning of August, our endemic climate fever began to appear, as customary. This fever is always periodical in its character. At least, in the course of twenty years' practice, I have never met it in any other form than that of intermittent or remittent, in their various modifications. A daily communication is kept up between Charleston and Savannah by steam-boats, and there was every opportunity afforded for a direct importation of the disease, if it preferred that mode of coming amongst us.

The fact is, that the Break-bone or Dengue fever was no stranger, although a good many years had elapsed since it had made a regular sojourn amongst us.

As brief a reference to its past history in this city as perspicuity will permit, may be interesting in more than one point of view; and I am the more inclined to make it, because, with all deference to so high an authority as Dr Dickson, I think he has somewhat misapprehended the views entertained of this disease by my esteemed preceptor, the late Dr Wm. R. Waring, of this city.

In Dr Dickson's now classic essay upon Dengue, published in Dr Bell's Select Medical Library in 1839, speaking of Dr Waring, he says, that Dr W. "makes his break-bone fever of 1826 identical with the autumnal fever of 1827, which again he looks upon as identical with yellow fever; thus mingling in promiscuous confusion, yellow fever, ordinary bilious remittent, breakbone and Dengue."—(P. 12.)

In the April number of the North American Medical and Surgical Journal of 1830, Dr Waring published an article on "the epidemics of Savannah," in the beginning of which we find the following sentences, which, by leaving out the words "novel and unknown here," might well be taken for a general description of the epidemic of last summer.

"We commence our account with the fall of 1826, which was dry and warm. It produced a species of disease novel and unknown here, which the people designated by the significant denomination of break-bone fever. *Its distinctive features consisted of violent pain of the head and loins, with a sort of rending of the bones at the extremities, of a single paroxysm of various duration, and a temporary inflammation of the skin and eyes.* Scarcely any person escaped, of whatever colour, age, sex, constitution, occupation, or habits; and yet notwithstanding this extensive prevalence, no instance of death occurred, at least of which I have been informed. The first cases excited great alarm; the fury of the symptoms, and early illness into which the patient seemed to be precipitated, presented the aspect of a disease uncontrollable and speedily destructive. A little experience, however, discovered that the pain and suffering very far surpassed the degree of peril, and that instead of being, as it threatened to be, an affection beyond remedy, it really required scarcely any remedy at all. It was an ephemeral gust, which did not destroy, but prostrated the muscles and the digestive function, and left behind it as great a sense of general debility as results from a tedious attack of autumnal fever."

A little further on, speaking of the diseases of the months of July, August, September, and October, 1827, he says,

"These latter portions of the year gave rise not merely at different times, but also at simultaneous periods, to some very extraordinary varieties in the prevalent fever. Perhaps two-thirds might have been distinct intermittents, while another third was composed of remittents nearly intermittent, of yellow fever in its worst degree, and of *genuine eruptive break-bone.*"

The distinction is here kept up most clearly, and the epithet *eruptive*, applied to the break-bone, is particularly significant. He further says,

"Cases of black vomit and break-bone were so minutely similar *in the first stage*, that you could not identify them."

This is not extraordinary to any one familiar with the excruciating pains in the lumbar region and the lower extremities which so often usher in attacks of autumnal fever, or indeed any severe febrile affection. Nay, this has induced some physicians in the southern country to look to the spinal column as the seat of the morbid derangement in our climate fevers.

It may illustrate this point, to state that in relation to these preliminary pains I have twice made a mistake in my own person, each time being wrong as to the disease. In 1828, while a pupil with Dr Waring, and when, as I shall detail hereafter, the Dengue was prevailing epidemically in Savannah, I was seized with some fever and such excruciating pains in my loins and lower extremities, that I was certain that I had the Dengue. I was mistaken. This was but the ushering in of a severe bilious fever, which nearly cost me my life. The events of that sickness are too firmly impressed on my memory for me ever to forget them.

As to my second mistake, I quote from my article on the epidemics of Savannah, published in the Charleston Journal for July 1849, when giving an account of the attack of Dengue, from which I suffered in the fall of 1848. When describing the commencement of the attack, I said,

"I felt an uneasy sensation down my thighs and legs, and a singular weakness in my knees. These symptoms increased so much on me, with the addition of a violent pain in the small of my back, that I hurried through my business and returned home, apprehending that I had the premonitory pains of a malarial fever."

Here was an error, again; these were the preliminary pains of Dengue.

Again, Dr Waring says, in relation to the year 1828,

"About the last of August, north-east winds resumed their usual predominance, and another *eruptive epidemic* was ushered in. It differed so slightly from the break-bone of 1826 and 1827, that in my notes I find the first cases called by that name, according to popular usage. This distinctive appellation, however, was soon supplanted by the Spanish word *Dengue*, which had migrated from Havannah to St Mary's, Savannah, and Charleston. The best explanatory denomination for common parlance, would have been eruptive rheumatism, or rheumatic eruptions, because it expresses the prominent form and features."

The last sentence is certainly as applicable to the epidemic of 1850, in our city, as it was to that of 1828. The next extract which I shall make from Dr Waring's paper, points out what differences existed between the epidemic of 1826 and that of 1828.

"I have said that Dengue was essentially the break-bone, which we had experienced in 1826, and also in 1827. The analogy was so striking, that I do not believe that we would have called it by another name, if it had not appeared in some other southern cities at the same time."

I beg to call attention to this testimony (published in 1830, but really written 1829), as to the identity of the two epidemics, then so recent.

To any practitioner who has seen cases of our recent epidemic, I think these quotations will prove that Dr Waring had noticed the peculiarities of Dengue with a careful and critical eye. It will be observed that he calls it "a disease novel and unknown here." He then details the symptoms distinctive of the disease, and points out wherein it differs from other diseases.

As to the pathology of the disease, Dr Waring was clearly misled by his devotion to the Broussaisan theory, which was then revolutionising the medical world. Every disease was a gastro-enteritis, in some shape or other. Dr W. was strong in his belief that this disease was a gastro-enteritis. Now that term in those days was applied to every disease in which the stomach was involved, never mind in what manner. This belief gave Dengue, in his view, some resemblance to other diseases besides yellow fever. After detailing a case, he says,

"The pathology of Dengue thus exhibited, *assimilates* curiously to three different diseases of gastro-enteritic foundation, viz., gout, erysipelas, and yellow fever."

Now it can hardly be thought that this is meant for more than to designate some points of assimilation. This assimilation is pointed out to uphold a theory, and it in no manner means to make this disease as *identical* with gout, erysipelas, or yellow fever. More to shew that this gastro-enteritic ground was merely theoretical, he says expressly,

"As no case of regular Dengue terminated mortally, it is not in my power to furnish any example of the morbid anatomy of its simple state, and to demonstrate, through this corroborating means, its gastro-enteritic character."

The resemblance to yellow fever is confined by him expressly to the first stages of the disease. Now, is it not always a difficult task to distinguish the peculiar nature of a fever in its very first stage? Are there not certain preliminary symptoms common to all fevers, indicating the general febrile disturbance of the system, but by no means what peculiar cause is producing that disturbance?

Here, then, I rest my retrospect of the history of Dengue, so far as this city is concerned, with the exception of its partial prevalence in 1848, as detailed in my article in the July number of the Charleston Journal for 1849.

On the morning of the 29th of August 1850, I was summoned in haste to see Mr T. M., a resident of this city. I found on my arrival that he had passed a restless and painful night, and that he was then suffering acute pain in the lumbar region, in the lower extremities, and in the head, from one temple to the other. The skin was hot and dry; the tongue coated with a thick white fur; the pulse accelerated and rather full. The reader will see in this description nothing more than what he has observed in many instances, of the ushering in of an ordinary climate fever. I prescribed a purgative of salts, senna, and manna. On my visit in the afternoon, the fever was as high as ever, and there was no abatement in the severity of the pain. The medicine had not acted freely, and as the head was much complained of, I directed a purging enema and a warm foot-bath.

On my visit on the 30th, the problem was solved. I discovered an eruption over the face and trunk, of red points, not minute like the flea-bite point of measles, nor in that crescentic form, which the aggregation of these points produces in that disease, but more resembling the eruption of scarlatina, in its interrupted form. The eruption was also attended with a tingling or itching sensation. I pronounced it at once to be the Dengue, or break-bone fever.

In the same row of buildings, a few doors off, the same train of events had simultaneously taken place with a child of about seven years of age.

Now I confess that, although from the fact that the Dengue was prevailing epidemically in Charleston, I had every reason to suppose that Savannah would not escape the epidemic influence; and that, therefore, I had been watching for some case of this disease, I did not suspect either of these two cases to be Dengue until the eruption made it palpable.

The symptoms were so analogous to those presented in

1848, two years antecedently, that there was no difficulty in settling positively the character of these two cases.

No new case occurred to me until the 4th of September. After this, the disease assumed an epidemic character, and spread in various parts of the city. As the chief point of interest in a description of this disease, is to settle its precise nosological character, I shall confine myself to a summary of the prominent symptoms developed in its appearance here.

The pains felt in all parts of the body, on the commencement of this disease, were very severe. In this epidemic, the pains were decidedly more muscular than arthritic. The muscles of the back were very much affected, as also those of the lower extremities. But there was universal soreness in all the muscles, rendering any change of position painful in the extreme. In infants, I very frequently diagnosticated the presence of true Dengue, from the evident pain produced by any motion of the body, or by simple handling.

It is easy to account for the name of this disease, as far as Break-bone is concerned, for in case after case have I heard the exclamation from the lips of the patient, "Oh, my bones are broken." Sometimes the pain was more predominant in one particular part of the body than in another. One gentleman declared to me that he felt that he must die, if I did not do something to relieve the oppression across his chest. Another complained of a sensation across his thighs, as if a heavy rail had been laid over the middle of them.

In the majority of cases there was a severe headache, shooting from temple to temple; an injection and watering of the eyes, and an intolerance of light, and acute and intense pain in moving the eye-balls. This was evidently owing to the affection of the sclerotic coat. Most generally the tongue was furred white, and there was a craving for cold drinks.

The pulse did not exhibit the disturbance that would be expected from the severity of the other symptoms. It was but seldom accelerated beyond one hundred; generally, it was but little over a hundred. I do not recollect of ever having met with it as high as one hundred and twenty. These preliminary symptoms were of longer duration in some than in others. They were followed, at various intervals of time, by an eruption, which, taken in connection with the anterior febrile disturbance, I consider as distinctive of the disease. I did commence to keep notes of the time at which the eruption appeared, after the first febrile symptoms; but the cases differed so materially in this particular, that I found it impossible to ascertain any definite rule. Sometimes, on my first visit, I found an eruption developed. Then again, it would become apparent the second day, or the third. When the eruption was tardy, it was very puzzling to know whether you were about to have a climate fever or the Dengue. The coats of the stomach were evidently more affected in these cases than in the others.

The skin was certainly the outlet of the system, by which the morbific matter of this disease was to escape, and hence, when any obstacle occurred to this, the internal organs were more disposed to suffer. The quicker the eruption made its appearance, the shorter was the sickness. The pains subsided very shortly after the appearance of the eruption, but the sense of debility left behind was really astonishing. Prior to the eruption, the fever was always continuous, never mind what time elapsed between it and the first seizing of the disease. In most cases, there was no return of the disease. The digestive functions were very much weakened, and, in some, there was an irritability of stomach which was very troublesome. In general, I should say that there was very pure debility of the stomach after the acute stage of the disease; for, contrary to Dr Waring's experience, I found that during convalescence, so far from producing any uneasiness, it was impossible to coax the stomach into digestion without the aid of some stimulus, either a vegetable tonic, a little weak brandy and water, or porter.

Those cases which were followed by irritability of the stomach, did not, however, bear this treatment. There was in these an evident derangement in the mucous coats of the stomach, and a disturbance in the functions of the liver. The tongue was wont to shew a bilious fur, and a mercurial purgative proved the most efficient means of restoring the digestive organs to their healthy tone. It was not until after this had been done that you could venture on the tonic and stimulant method.

Now, in this secondary affection of the stomach and liver, I see nothing more than a complication arising from the general predisposition to gastric disease, which always exists in warm climates during the summer and fall months. It was not distinctive of the disease, as it existed only in a small portion of those affected. But the eruption was distinctive, and I never considered a case one of true Dengue, unless this made its appearance. Another distinctive feature of this disease was, that the pains were evidently neuralgic in their character; for even when they lasted twenty-four, and thirty-six, and forty-eight hours, they did not produce that disturbance of the general circulation which they would have done had they been of an inflammatory nature.

Now, when reference has been made to the difficulty of deciding between an attack of Dengue, and one of climate fever, it must be recollected that, in common with Dr Waring, I confine it to the first stage of the Dengue. Remission or intermission would undoubtedly take place in climate fever in the course of twenty-hours. But the paroxysm of fever ushering in Dengue was a continuous one, even if it lasted forty-eight hours.

That after the distinctive eruption has appeared, the patient has suffered intermittent or remittent fever, does not affect the question; for an attack of the Dengue afforded no exemption from an attack of climate fever. I treated many patients, during last season, who had first had Dengue, and after an interval of two or three, or four weeks, suffered from climate fever, and *vice versa.* If, then, we have a disease, which, as a rule so very general as to admit of few exceptions (and to which I myself know of none), is ushered in by severe muscular and arthritic pains, one or both, and by a continuous paroxysm of fever, and ending in a cutaneous eruption, why should it be even assimilated to climate fever? In many cases under my treatment, the patient, after the eruption, has improved so much that I have ceased to attend, thinking him quite well; but the next day I would be summoned back, and find my patient quite sick, with fever and gastric disturbance. I always considered and treated these cases as complications of climate fever, because they always gave more or less evidence of gastric and biliary derangement, and invariably assumed a remittent or an intermittent form.

During the whole existence of this epidemic amongst us we had our usual endemic autumn fever, assuming as common, either an intermittent or remittent type. I have attended cases of each lying alongside of each other, among children of one family, and also among the poorer classes of whites, (foreigners), who huddle together in an extraordinary manner; and yet each has run its course unmodified by the presence of the contiguous disease.

The eruption, as far as I saw it, was only of two kinds, one in patches, the other diffused. Both were smooth, until the cuticle began to dry and desquamate. Some few cases of eruption were as red and diffused as I ever saw in true scarlatina. In all, the eruption could be pressed back by the fingers, leaving a white mark until the blood rushed back into the capillaries. By this mode I convinced very many persons (white of course) that they had an eruption; for, in some instances, the blush of the skin was very faint, but pressure would prove, by the superior whiteness left under the finger, that there was an injection of the vessels of the periphery. This also proves that the cutaneous inflammation is seated in the outer or cuticular surface of the *cutis vera.*

The duration of the eruption, and the absence of any anginose affection, as well as the violence of the preliminary symptoms, distinguish it from scarlet fever. The eruption in Dengue was the sign for the abatement of the symptoms. So far from this being the case in scarlatina, we know that generally the worst symptoms are developed after the eruption has appeared.

In scarlatina the skin never breaks out into a perspiration on the first appearance of the eruption. In Dengue, after the eruption has been well thrown out, the fever goes off by moisture on the skin. The eruption lasts but two or three days in Dengue; in scarlatina, it lasts six or seven, hence there is decidedly more desquamation of the cuticle in the latter than the former. The tongue also affords a guide. In the commencement of the eruptive stage of scarlet fever the mucous membrane of the mouth participates in the efflorescence, and as the external skin begins to throw off its cuticle, the cuticle of the tongue is also thrown off, and the tongue appears bright red and shining; and the papillæ, being deprived of their cuticular sheath, become very prominent on its surface. When scarlatina prevails epidemically, it is by the tongue that I diagnosticate this disease among our blacks. Whatever redness of the lips, tongue, and gums, may take place in Dengue, it is not, as far as any case has come under my observation, ever likely to be mistaken for the redness of scarlatina; for it is less intense in colour, lasts a much shorter time, and is not followed by that throwing off of the cuticle of the tongue, which is so apparent in this last disease. In two cases occurring in children I saw general œdema follow the desquamation of the cuticle, exactly as it does in scarlatina. They were both very fair, thin-skinned children, and I suppose the skin was in them more than usually sensitive.

The heat of the weather produced an unusual quantity of prickly heat; (*lichen tropicus.*) Any one who has seen the two eruptions would not readily confound them, if he should examine with any care. Prickly heat is a distinct papular disease, that is to say, consists of hard and solid elevations.

I was very inaccurate in my article of July 1849, when I spoke of the eruption as being papular in those cases occurring in the fall of 1848, and I must have decided too hastily, for now I plainly see the difference between a real papular disease and the eruption of Dengue. I meant to express the fact, that the eruption occurred in points or patches, as in measles, and not in the uniform diffuse redness of scarlatina. But this was not the case during the last summer, for many of the cases presented a uniform, scarlet-red diffused eruption. There were also an uncommon quantity of boils last summer, among grown persons as well as children. Indeed, I think I might safely say that they were entitled to be considered epidemic also.

My treatment of this disease was very simple. I purged with Epsom salts, either plain or in infusion of serpentaria; directed sinapisms when the pains were very intense, and hot foot-baths; sometimes I gave antimonials as sudorifics, never as emetics; to allay thirst and produce perspiration, I ordered hot lemonade; after the bowels had been moved, I used opiates very freely, and with most decided alleviation of the painful symptoms; where there was no irritability of the stomach, I preferred Dover's powder to any other. I am satisfied that not one quarter of the cases that occurred were treated by physicians. The disease was very general, and I believe that very few in the whole city escaped it. This is satisfactory proof, that this epidemic was as harmless in its results upon life as its predecessors have been. I know of no instance of a death having occurred from simple uncomplicated Dengue. But the suffering in many cases was very great, and I am satisfied that it was very greatly alleviated by remedies. Moreover, it was much to be aware that the violence of the symptoms was no evidence of the danger of the disease, and hence we were enabled to avoid heroic depleting remedies, and to preserve for the patient as much strength as possible, for what always proved a tedious convalescence. Indeed, I must consider the extraordinary debility left after an attack of this disease (it being so comparatively short in its duration), and the slow and tedious convalescence, as peculiarly characteristic of it.

And now the question occurs, what is Dengue? I have no doubt that it ought to be classed as a specific exanthematous fever. The variations observed in it, in its epidemic existence at different times, are no greater than those which occur in scarlatina or rubeola, at various times of their prevailing. Its variation, as it occurred here in 1828 and in 1850, consisted in the fact, that in the former year it was more arthritic.

Dr Waring, in the article cited above, speaks of the alleviation of the pain, " after a bright red eruption had made its appearance." He also asserts that " the eruption was developed in most cases;" and that in " some cases it spread in patches; in others, in an unbroken blush, over a greater or less extent."

Here, then, is a disease presenting peculiar symptoms, viz., a febrile disturbance, and violent pains in the limbs or joints, or both, preceding an eruption of a bright red colour, at intervals of time somewhat varying, but seldom exceeding forty-eight hours; this eruption of a determinate duration in the majority of cases.

At the same time that this disease prevails there prevails also another disease endemic to this city in the summer and fall; cases of each are seen in juxtaposition, and each presents its distinctive peculiarities. Whatever points of assimilation there may be between them, however the digestive functions may be disturbed in each, there is no identity.

I know that some physicians, when an epidemic prevails, consider that all diseases then occurring are modified by its influence. This is true to a partial extent. But the effect is the development of the disease itself on the least exciting cause, and not the development of a hybrid compound. Now during the last season I treated a great number of climate fever, intermittent and remittent. There was not the slightest difficulty in deciding upon their character and treatment, after sufficient time had elapsed to shew their periodicity. But, when I was first called to a patient, and was asked " Have I got the Dengue?" my answer invariably was, " I will tell you in a day or two." If the eruption did not appear, I did not consider it a case of Dengue. On this point the epidemic shewed some variation of symptoms from what it did in Charleston; for I infer from Dr Dickson's recent article on Dengue, that the eruption was deficient in a great number of cases there. I say positively that I met with no case which I thought entitled to be considered Dengue, in which the eruption did not appear at some period. I am also inclined to think that this disease appeared in a more simple form here than elsewhere, from the fact that Dr Campbell, of Augusta, styles it a " truly Protean epidemic," as seen in that city. Beyond that variety of symptoms which any disease shews in different individuals, I could not apply this term to the epidemic here. On the contrary, it was marked and distinct. Of course some were affected more violently than others, presenting variety as to the intensity of pain, and as to the affection of the digestive functions. Many persons who had the disease here did not go to bed.

There are two circumstances about this disease, as it has shewn itself in our city, which merit consideration.

First, It has shewn itself here to be a city disease, as yellow fever is. It was extensively epidemic here in 1826, and 1828, and 1850. Almost everybody in the city had it. Speaking of the epidemic of 1828, Dr Waring gives us the following distinct testimony.

"I have said that Dengue was essentially the break-bone which we had experienced in 1826, and also in 1827. The analogy was so striking that I do not believe we should have called it by another name, if it had not appeared in some other southern cities at the same time. The points of resemblance were the universality of both diseases, the same gradual progress, by which the entire population became involved, excruciating pain of the limbs and loins, the same species of cutaneous inflammation, singular violence and short duration both, their common characteristic of forming but one paroxysm, red injection of the vessels of the conjunctiva, uniform recovery."

In 1827 our city was visited epidemically by yellow fever, its last epidemic visitation. Further on, he says, referring to the years 1826, 1827, and 1828, "As regards yellow fever, break-bone, and Dengue, the remote cause confined itself to the boundaries of the city proper."

This has not been the case with scarlet fever and measles. They have prevailed extensively on plantations. Now, although Dengue prevailed for a series of three years, viz., 1826, 1827, and 1828, in the city proper, and during two of those years, was very general, we here have distinct testimony as to its being confined to those limits. The same occurred in 1850. For aught I know a few isolated cases may have got into the country surrounding us, but the disease certainly never became epidemic there.

Dr James P. Scriven, of this city, who was a practitioner in 1826 and 1828, in a conversation with me, mentioned the fact of the confinement of this disease at that time to the limits of the city, as a peculiarity in it. He thus corroborated the observation of Dr Waring. Dr S. is one of the most extensive planters on the Savannah river, and he stated that he was not aware of the occurrence of a single case on any of his plantations, during the past year.

Second, This disease is undoubtedly affected by frost. The diminution of cases after a frost last fall was as marked as the diminution of cases in our endemic climate fever usually is. Persons who had the seeds of disease latent at the time became affected afterwards. About the beginning of November cases became quite rare, but a case was occasionally met with. On the 18th November I saw the last case of the season, as far as regards my experience. It occurred in an infant eight months old, who had been absent during the summer, but had returned about the end of October.

How did this disease originate? When a definite answer is returned, as to the origin of smallpox, scarlet fever, measles, or hooping-cough, I shall expect one to this question.

I have not aimed at giving any particularly graphic description of this disease, as it appeared here last summer. I have been struck, however, with the fact of its former epidemic prevalence here, in 1826 and 1828. A careful perusal of Dr Waring's admirable article has convinced me of the identity of these former epidemics with the one of 1850. Both Dr Waring and Dr Daniell, of this city, as quoted by Dr Dickson, contended for the identity of the break-bone of 1826 and the Dengue of 1828. As eyewitnesses, and as having given their testimony many years ago, their evidence cannot be set aside to suit any theory. Dr Dickson, in his first article on Dengue, dwells with emphasis on the testimony of Dr Stedman, as to its starting-point in this hemisphere being at St Thomas's in 1827. How it happened to come to Savannah in 1826 will, I suppose be solved at the Greek calends. Dr Dickson acknowledges the identity of the epidemic of 1850 with that of 1828; and Dr Waring and Dr Daniell, the identity of that of 1828 with that of 1826. Here is the chain of evidence complete. It certainly did *not* come to Savannah from St Thomas's in 1826. The truth is, that there is a singular propensity in communities to blame other communities for the origin of epidemic diseases. If I mistake not, in Charleston the scape-goat for yellow fever is Havannah. Now, on the 1st day of December 1849, I treated a decided case of yellow fever, terminating in black vomit and death, which had been contracted in Charleston by a foreigner, who had stopped there two weeks on his way hither. I was called in on a Saturday. The man had arrived the previous night in one of the Charleston boats. On Sunday he threw up black vomit, and on Monday he died;—and there was the end of it and him. Now, had this case occurred early in the season, and had others occurred after it, the contagionists would have blamed Charleston for all such cases. I should not; because in 1839 I saw two cases of yellow fever, ending in black vomit, occurring under circumstances which ought to have favoured its spreading. Yellow fever prevailed epidemically in that year in Charleston and in Augusta. At the latter end of August of that year a man who had just left Charleston, entered the hospital, the wards of which were then filled with cases of bilious remittent fever. He died with black vomit. About ten days afterwards another man entered, who had just left Augusta. He died with black vomit.

If these patients did not bring the disease hither with them it was sporadic here; if they did, it certainly failed to propagate itself, under circumstances highly favourable to such propagation,—but not a single patient contracted the disease. Now, had there existed an epidemic tendency to this disease, and had it broken out shortly after the occurrence of these two cases, they would have been considered irrefragable evidence of its contagiousness. That a peculiar disease, like yellow fever, failed to propagate itself, when introduced into the wards of a fever hospital, in the very season of the year when climate fevers are endemic, is surely entitled to be considered more than negative evidence on the subject of its contagiousness.

My first cases of Dengue or break-bone occurred the 29th of August. This disease had been prevailing epidemically in Charleston for more than a month. There is a daily communication between this city and Charleston by steamboats, going from wharf to wharf. Here certainly was a means of communicating contagious matter. But no one in our city made any movement towards quarantining Charleston.

Neither of those cases had had any communication, direct or indirect, with Charleston. The disease was not then known to exist here. There was no centre in our city from which to trace their radiation. They were owing, according to my belief, to an epidemic influence. Persons say that means nothing. I differ from them. Those words express a fact, however unable we may be to solve the ultimate cause of that fact.

The Dengue last year was really epidemic. Persons from abroad, who came within our limits, were seized with it, without ever having been near anybody sick with it. I detailed, in my article alluded to before, the case of W. D., Esq., and my own, which occurred in the fall of 1848. Each was the only case in our respective families. No precaution was taken for isolation, in either case; yet no other individual of our families contracted the disease. Is such a fact of no importance, in deciding upon the contagiousness or non-contagiousness of this disease? I confess that I cannot account for the origin of the first cases, on the theory of contagion; nor did the disease spread in a manner to induce such a belief. It did not, to be sure, burst out at once in every part of the city; but it did break out at various points, within a very few days of the first cases, and, by the middle of September, it had visited every portion of it.

Fortunately, this disease, which seems to prevail so generally when it does appear amongst us, is one of no fatality. Tedious convalescence, and, in some few instances, one of which is now under my care, chronic arthritic rheumatism, are its only effects.

ART. V.—*Description of the City of Bahia; its Medico-Topographical Relations; its Salubrity; the Causes of that Salubrity.* By ROBERT DUNDAS, M.D. (Sketches of Brazil, &c. London, 1852. Lecture V., Pp. 200–285.)

The city of Bahia, the former capital of Brazil, is situated in latitude 12° 59′ S., and longitude 38° 33′ W., at the entrance to one of the most beautiful and extensive bays in the world, extending thirty-three miles in length, from north to south, and thirty miles in width, from east to west, and containing upwards of a hundred islands, several of which are inhabited. The city contains about 150,000 inhabitants; the population in 1803 being estimated at 103,000. It may be said to form two divisions, the one constituting the upper, and the other the lower city. The former division is built on a ledge of rock (chiefly gneiss, passing often into grey primitive greenstone and syenitic granite), about six hundred feet in height, and overhanging, as it were, the lower division. The latter, built on an alluvial soil, with a rocky substratum, stretches along the base of the hill, on ground gained in part from the bay.

The streets are irregular, ill-paved, generally narrow, and having a gutter in the middle, into which is commonly cast the filth and offal of the adjacent dwellings. The houses are unprovided with water-closets, and are otherwise ill-arranged for the purposes of ventilation and comfort. The slave population is numerous and crowded, the police inefficient, and a scavenger department unknown. The public sewers are few, the *becos* (alleys) leading from the principal streets serving as temples of Cloacina, and as receptacles for every kind of filth. In addition to this, the dead are interred within the churches and precincts of the city. I am, however, informed, that since my return from Brazil in 1842, the state of affairs here alluded to has undergone considerable amelioration through the advancing enlightenment of the age, and the more energetic efforts of the public authorities.

Partial, though heavy rains, occur at all seasons of the year, by which the lower city is inundated, and contaminated by the filth and offal of the upper city. In some of the most public thoroughfares, as the "Conceiçao," for example, there are heaps, or rather mounds, of decaying animal and vegetable matters fermenting under the powerful rays of a tropical sun, disengaging every kind of noisome effluvia, and often of such an offensive character as to impress the sense of taste scarcely less powerfully than that of smell. The soil, too, in the immediate neighbourhood of the city is absorbent, deep, and rich, with a substratum of clay. Here, then, we have, accumulated, in almost unexampled abundance, all those physical conditions which are deemed, by the unanimous consent of physicians, to constitute the elements essential for the generation of the most deadly scourges of humanity;—epidemic and endemic diseases. I may further add, that I have myself, within the last twenty years, witnessed the city exposed, on three several occasions, to the combined horrors of siege and famine, with all their revolting contingencies. Yet, notwithstanding this appalling combination of physical, moral, and social evils, universally admitted as the chief agents in producing the most extensive and fatal diseases, Bahia continued healthy; and can, moreover, up to the present hour, boast the happy privilege of having escaped, since the period of its foundation, from every species of endemic or epidemic malady,—*yellow fever*, *cholera*, *influenza*, *typhus*, and *dysentery*.*

It is, moreover, deserving of notice, as regards the general health of the inhabitants of the upper and lower divisions of the city, that the Brazilian and Portuguese merchants and shopkeepers, who inhabit the lower divisions, and who rarely quit their dwellings, enjoy comparative immunity from intermittent fever; while those whose duties frequently lead them from the lower to the upper city, often suffer from this disease, as well as from continued fever *(constipacōn)*, without other apparent cause than the sudden transition from the warm, but equal temperature of the lower city, to a strong, cool, and humid sea-breeze, which they encounter while bathed in perspiration and exhausted by the labour of ascending a considerable eminence.

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* Since these pages were written, a desolating epidemic, said to be the *yellow fever*, has swept over the chief cities of Brazil. It is, I believe, generally considered by the profession to have been imported from abroad; but I trust that ere long we shall be supplied with full and accurate information as to its origin, progress, symptoms, and treatment. It would appear, from the writings of an old Brazilian historian, Sebastiano da Rocha Pitta, that, in the year 1686, a somewhat similar epidemic committed great ravages in the cities of Bahia and Pernambuco, and presisted for five or six years. The fact is also mentioned by Humboldt.

How, then, are we to explain this most singular salubrity of the city of Bahia? For while most of the ordinary endemic and epidemic diseases are found to occur sporadically, and sometimes in their most intense and deadly form, yet it is matter of history that they have never spread endemically.

Equability of Temperature.—I am inclined to believe that the true solution of this remarkable phenomenon is to be found in the extraordinary equability, and limited range of the temperature, the prevalence of a gently but never-failing breeze, and the freedom from atmospheric vicissitudes enjoyed by Bahia, beyond any other city with which I am acquainted. *The highest range of the thermometer in the upper city (with the exception of an occasional day,) never exceeds 82½° of Fahrenheit in the summer, and the lowest in winter is 72°; there is a further difference of two degrees between the upper and the lower city; and the extreme daily range is about six degrees.* This unexampled uniformity of temperature is chiefly to be ascribed to the absence of any high or mountainous ranges, and of all arid and sandy deserts, aided by the genial influence of refreshing showers at all seasons of the year. It is further promoted by the perpetual verdure of the country, and by a cool and powerful monsoon, laden with moisture, and sweeping along the coast direct from the southern Atlantic. This monsoon prevails from the north-east for eight months of the year, namely, from September to April, constituting the dry or summer monsoon; and during the winter or rainy season, from May to August, it blows from the south-east. These periods are, however, liable to some irregularity.

The nights in Bahia are also usually serene and beautiful, and unattended with much deposition of dew. Unlike what occurs in other hot regions, the delightful serenity and coolness of tropical moonlight may be enjoyed with perfect impunity, the mind being undisturbed by those visions of fever and malaria which float before the imagination in less favoured climates. Of this we have the best practical illustration in the number of foreigners, as well as natives, at Bahia, who pass a large portion of the summer nights in the open air.

Another subject of vast importance to the European in tropical climates must not be forgotten; namely, the sound and refreshing sleep which may always be enjoyed at Bahia; the nights being invariably cool, even in the midst of summer. If proper precautions be observed to avoid exposure to direct currents of air, the windows of the sleeping chamber may be left open with impunity at all seasons of the year; the terrors of malaria, and the hot, suffocating winds of other tropical climates, being altogether unknown. I am not, indeed, aware of any circumstance which exerts a more important influence in enabling the European constitution to resist the deleterious effects of tropical climates, than the enjoyment of sound and refreshing sleep.

Condition of the People.—Among other causes which must be held as important in modifying the general character of diseases in Bahia, and probably everywhere else, the physical, social, and moral condition of the people deserves special consideration. The native Brazilian is in general compact and well-formed, and of healthy organisation, but not of an athletic frame. His intellectual faculties are acute, though little developed by cultivation. Descended from European ancestors, he has still a considerable admixture of African and native American blood. He is indolent by nature, and indisposed for active exertion or industry; but he is protected against the evil influence of the former on his health, by a simple and abstemious diet; and the injurious consequences of the latter to his social position are obviated by the circumstance, that the four great wants of the humbler classes in Europe press but lightly on the Brazilian. Fuel he scarcely requires; of clothing but little; his primitive habitation is simply constructed; and one day's labour will amply provide for the moderate demands of the whole week. With passions naturally quick, he is nevertheless placable; his disposition is kindly; the future never disturbs him with its doubts, nor the past with its regrets; the struggles and vicissitudes of European life are unknown. The contentions of party, the yearnings of ambition, the bitterness of fanaticism, never disturb his repose; and after gliding down the stream of time, unscathed by those tumultuous passions and harassing cares which so frequently embitter the existence and undermine the constitution of man in other countries, he meets at length the inevitable doom, if not with philosophy, at least with resignation; satisfied of his claims to eternal felicity, in the confident assurance of an infallible church.

Such is, or rather was, the Brazilian. I speak of the masses; for the higher classes in all countries pretty closely approximate. But the premature, though well-intentioned, introduction of political institutions, unsuited to the material interests or intellectual advancement of a mixed population like that of Brazil, has already operated, and will probably continue to effect still more important changes on the habits and social condition of the people. These questions, however, important as they are, I must leave to the political economist and the historian. We have only to do with the past and the present, in our attempts to elucidate the causes to which Bahia has hitherto owed her singular and happy immunity from those endemic and epidemic scourges which too frequently desolate some of the fairest regions of the earth, and where, from similarity in geographical position, the inference of similarity in disease would appear to be almost legitimate.

From the preceding account of the climate and inhabitants of Bahia, we should naturally be led, *à priori*, to conclude that disease would there present a mild and tractable character; and it is highly interesting to find that in such conclusion we are here borne out by the concurring evidence of experience.

STATEMENT OF FACTS WHICH FIRST LED THE AUTHOR TO DOUBT THE CAUSAL RELATION OF MARSH MIASM TO INTERMITTENT FEVER.

In the year 1819, I was appointed to the British Hospital at Bahia. When I commenced the duties of this establishment, it was reported to me that the locality was considered unhealthy, as patients received into the Hospital were afterwards subject to be attacked, during convalescence, with intermittent fever. Reports of this nature necessarily caused me uneasiness; and I accordingly investigated with much attention all the internal arrangements of the Hospital, as well as the immediate neighbourhood. Nowhere could I discover any obvious causes of intermittent fever. There were no marshes; and the Hospital, built on an eminence, had well-arranged and spacious rooms, and was fully exposed to the current of a strong and regular monsoon or sea-breeze. Such being the case, and my mind being fully imbued with the universally-received notions regarding the origin of intermittent fever from marsh poison, I discredited the information as totally unworthy of notice. In a few weeks, however, after I had entered on the duties of the Hospital, my attention was unpleasantly arrested by the occurrence of a well-marked case of ague, the history of which may be here briefly related.

1. *Case.*—A seaman in the prime of life, and in robust health, was admitted into the Hospital, labouring under viru-

lent gonorrhœa, which demanded the employment of very active antiphlogistic measures, including general and local bloodletting, very low diet, &c. In the course of three weeks, and when the acute symptoms had entirely subsided, the patient was suddenly seized with a violent shivering fit, which lasted some hours, and was followed by strong reaction; the paroxysm being carried off by profuse perspiration. This incident struck me forcibly at the moment. I attributed it, however, notwithstanding the protestations of the patient, to some imprudence in diet or otherwise; and, perceiving that the fever gradually subsided, I felt satisfied of the justice of my suspicions. On the third day, however, and precisely at the same hour, the patient was seized with a paroxysm exactly similar to the former, only much more violent. In short, intermittent fever was established; and it continued, notwithstanding the administration of ordinary remedies, until the period of the patient's embarkation in his vessel for England, about five weeks after his admission into Hospital, and twelve days after the commencement of the ague. This case caused me much embarrassment at the moment, as inexplicable by the received doctrines,—the more so, as the patient had not only never suffered from ague, on any previous occasion, but had never, according to his declaration, been conconfined for a single day by illness during the whole course of his life.

Another case occurring shortly after the above, afforded further grounds for reflection.

2. *Case.*—A seaman, likewise in the prime of life, was admitted into Hospital with severe rheumatic inflammation of the shoulder-joint, brought on by exposure to wet and cold during the voyage from England. This man was of a full plethoric habit; the shoulder was red, inflamed, and considerably swollen; and there was strong constitutional disturbance. Under these circumstances, a rigorous antiphlogistic regimen, including general and local bloodletting, &c., was adopted. By these means, the local and constitutional symptoms were rapidly subdued, and the patient was pronounced convalescent on the seventh day. On the morning of the tenth day, while examining another patient in the ward, I was called to this man, and found him trembling violently, and exhibiting all the ordinary appearances of the cold stage of intermittent fever. The disease was arrested after the third paroxysm by the exhibition of cinchona bark.

It is unnecessary to multiply cases; they were unfortunately but too numerous; and I quickly learned, by the painful tuition of experience, that, in the British Hospital, patients convalescent from any disease,—more especially from rheumatism,—whose strength had been much and suddenly lowered, became exceedingly obnoxious to attacks of intermittent fever, which proved almost invariably of the tertian type.

There was not a deficiency of cases of the same character in my private practice. The following may be offered as an illustration.

3. *Case.*—Mrs D., wife of a British merchant, lately arrived from Europe, in the prime of life, and married but a few months, had been out at a *fête champêtre* on the sea-coast. She danced and perspired freely; and after the excitement of the day was exposed in the cool sea-breeze of the country, while returning home in his *cadeira,*—a sort of palanquin peculiar to Bahia. The result was a sharp attack of inflammatory fever, apparently *gastro-enterite,* which required a strict antiphlogistic regimen, but no bloodletting. On the seventh day, Mrs D. was deemed convalescent; she was so perfectly free from all ailment, that I informed her husband that every source of uneasiness had now been removed. Scarcely six hours, however, had elapsed after this declaration, when I was hastily summoned, and found Mrs D. in a state of high febrile excitement, having had previous rigors of nearly two hours' duration. I perhaps naturally enough, notwithstanding the strenuous denial of the patient, ascribed her relapse to some irregularity of diet or other imprudence. My friend and colleague, Dr Silveira, physician to the Mizericordia Hospital, and subsequently physician in ordinary to the Emperor, was called in consultation, and was as much embarrassed as myself satisfactorily to account for the sudden accession of fever. We prescribed some saline medicine, and on the following morning were gratified to find our patient, after copious perspiration, free from fever, and complaining of nothing beyond debility. We accordingly again pronounced her convalescent. On the afternoon of the following day, however, at the same hour, and almost at the same minute, an attack, almost similar to the preceding one, came on; in short, intermittent fever was duly established, and the patient was restored to health in the course of two or three weeks by the ordinary tonic treatment.

Village of San Lazaro.—I shall now direct attention to the history of the *Povoação,* or small village, of San Lazaro. This place is situated about two miles southward of Bahia, on the verge of the peninsula, close to the sea-shore, on a bold headland, and fully exposed to a strong and regular monsoon. The country around is very beautiful; the sea-view magnificent. It is therefore chosen as the favourite evening ride of the British and other foreign residents. It is also frequently resorted to for the day by parties of pleasure, who return to the city in the evening. In this village there was only one good house, which belonged to a Portuguese merchant, and which being unoccupied by the owner, was always at the disposal of such foreign visitors as might require it. The house was large, situated on the brow of a hill, and facing the sea. A spacious open verandah extended along the entire front of the building, through which the cool sea-breeze rushed with great force. Nevertheless, the house was by universal consent deemed very aguish, as was also that of Colonel Cid, which stood on a neighbouring eminence; while some small cottages at the base of the hill, and sheltered in a great measure from the sea-breeze, were considered healthy.

Mr R., one of the resident British merchants, requested my opinion regarding the character of the house in question, as he was desirous of removing his family there for the purpose of sea-bathing. Guided by the same principle as had influenced my decision regarding the healthy character of the British Hospital, I stated that there appeared no rational grounds for giving the house so bad a name; and, acting on this opinion, Mr R. and his family removed there on the following day. This gentleman's household consisted of himself, Mrs R. his wife, Mr M. his brother-in-law, a youth of 17 years of age, recently arrived from England, an English houskeeeper, and five black servants.

On the fourteenth day after their removal to San Lazaro, the state of the household was as follows:—Mr and Mrs R. and one of the black servants were laid up with ague; another servant with dysentery; Mr M. a severe attack of inflammatory fever, with great determination of blood to the lungs; and Mrs C. the housekeeper, a woman of melancholic disposition, and weak leuco-phlegmatic habit, was attacked with genuine typhus.

The whole establishment was consequently removed back to Mr R.'s house in the city, where Mr M. advanced quickly to convalescence under active antiphlogistic treatment, as did also the two black servants under the ordinary measures. Mr and Mrs R. recovered more slowly, but without any serious inconvenience; while the condition of Mrs C. the housekeeper became perilous in the extreme. She had a weak and rapid pulse, intense heat of skin, oppression at the chest, dry tongue with dark fur, and sordes about the teeth and lips, tenderness of the abdomen, offensive and depraved secretions and excretions, low muttering delirium and petechiæ on the surface of the body. This patient finally recovered, but with great difficulty, having been confined to bed during thirty-three days. My friend Dr Silveira, saw this case repeatedly in consultation, and admitted that he had never witnessed a more exquisitely marked case of typhus. Of this he was certainly a most competent judge, having graduated at Edinburgh, where he resided several years, and performed the duties of clinical clerk at the Royal Infirmary. He is, moreover, a gentleman of great professional talent and scientific acquirements, and well merits the high office to which he has since been appointed by the Imperial Government.

The case just described sufficiently proves a fact which has often been questioned, particularly by Bancroft—the possibility of the occurrence of typhus, genuine typhus, in tropical climates.* And the different diseases from the same exposure, with which the several members of the above family were assailed, afford a striking illustration of the influence of secondary causes in determining the character of the subsequent disease. Mr M., young, robust, and just arrived from Europe, had an inflammatory attack, with determination of blood to the lungs ; Mr and Mrs R., who had resided for some years in the country, and had suffered somewhat from climate, were affected with ague ; Mrs C. the housekeeper, who had also been some time in the country, and who was weak and of a melancholic and leuco-phlegmatic temperament, was attacked with typhus ; and of the black servants, one suffered from ague, and another from dysentery.

On a previous occasion, while serving on the medical staff in the unfortunate expedition against New Orleans in 1814–15, I also witnessed a broad illustration of the above principle. Returning from the army with some officers of H. M. 14th Light Dragoons, we were exposed for three or four days in a frigate's launch to cold and wet, and other privations. On reaching the fleet at Cat Island Roads, all except one were suffering, and each from a different malady. The midshipman who commanded the boat was attacked with otitis, and became perfectly deaf; one of the officers was attacked with dysentery, another with ophthalmia, a third with fever, and myself with rheumatism, to which I have since, up to the present hour, been occasionally liable. And all these diseases originated in one and the same cause.

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* In some seasons, sporadic cases of typhus, genuine typhus, are by no means rare in Brazil, and are popularly termed "*febres malignas.*" They commence either as ague, or as remittent fever, or as a "constipacao," (in Portuguese), which resembles the synocha of Cullen. These cases are often fatal, and commonly run a more rapid course than the typhus of Europe. The "constipacao" or continued fever, is a frequent, perhaps the most frequent, form of fever in Bahia, and usually runs its course in from five to seven days, terminating in free perspiration. The "constipacao" is rarely or ever fatal, unless, as before stated, it should run into "maligna ;" but it not unfrequently lapses into ague, which commonly assumes the tertian type, and yields readily to treatment. The so-called typhoid fever is also not unfrequent, especially at Rio de Janeiro, and has been well described by Mello *Franco*, (*Ensaio sobre las febres do Rio de Janeiro*, Lisbon, 1822,) and subsequently with its abdominal lesions by Sigaud, physician to the Emperor, and by Drs Valadao and da Costa, as it appeared in their hospitals of the Mizericordia and the Marine.

Indeed, we find that the diseased action, apt to follow on the application of any of the ordinary causes of disease, often depends less on the special nature of such cause than on the internal condition of the animal and organic functions, both at the time of application of the cause and for some time previously. There are few diseases, perhaps none, which can be said to arise instantaneously; and it is more than probable that the preceding condition of the individual, and the state of more or less marked *malaise*, which is to be detected on close investigation, must essentially contribute to guide and direct (if I may use the expression), the exciting cause to the development of one form of diseased action in preference to another. Thus the type and symptoms of fever will assume different forms at different seasons, and under different circumstances. At one time they will be displayed through the air-passages; at others through the nervous system, the alimentary canal, &c.; yet, strange to say, a difference in the febrile poison has even been assumed to account for these several varieties. Names and theorems, have too often usurped the field of observation, setting aside the wide and unbounded expanse of nature as daily presented to our notice, ungarnished by the sophistry of the schools, or by the dialectic craftsmen, who labour to bend, twist, and torture natural phenomena to the favourite theory of the closet, and, like Procrustes, endeavour to reduce each and every form of diseased action to their own standard.

Dr Dundas's Case.—In the year 1822, I became myself the subject of intermittent fever. The ague arose as follows. Our envoy to the court of Brazil having stopped for a few days at Bahia, I had occasion to accompany him on some excursions around the city; during which, in addition to my professional duties, I was much exposed to the sun. On one day in particular, after lengthened exertion and exposure, I felt exhausted, and went into a tepid bath previously to dressing for dinner. Finding the bath more than usually grateful, I continued in longer than was my custom, and until warned by a slight sensation of cold. I then quickly dressed and went out to dinner in my ordinary health. During the night, however, I was seized with a violent shivering, which terminated in tertian ague of the most obstinate character, resisting every mode of treatment for a period of fifteen months. My health finally became so seriously deteriorated, as to make it imperative that I should have leave of absence, in order to proceed to the Organ Mountains in the province of Rio de Janeiro for change of climate. This I was enabled more readily to accomplish through the kindness of the commander-in-chief on the station, the late gallant Sir Thomas Hardy, who not only afforded me a passage in H.M.S. "Fly," but permitted his flag-surgeon, Mr Neill, and subsequently Dr Birnie, of H.M.S. "Tartar," to take charge of the British Hospital during my absence; and, though late, I am happy in being afforded the opportunity of acknowledging my deep obligations to these gentlemen.

Probable Cause of Ague.—Though, up to this period (1822), I was in possession of facts sufficiently strong to raise serious doubts in my mind as to the source of intermittent fever, I nevertheless adhered to the orthodox doctrine, that ague originated only from the agency of marsh miasmata. My faith had however become wavering, and whilst in this state of suspense, I was called to attend the family of an intelligent Brazilian merchant residing in the *Saude*, whose wife and two children, the latter aged respectively seven and ten years, were affected with intermittent fever. Their house was spacious, in one of the highest quarters of the city, and fully exposed to a strong sea-breeze, a locality which would, *à priori*, be pronounced unexceptionable, and especially exempt from all chance of *malarious agency.* The occurrence of three cases of ague under such circumstances (the more remarkable, as the female members of Brazilian families but rarely quit their habitations), afforded additional and very strong reasons for further questioning the received theory of the origin of ague from malaria. In conversation, I stated my doubts to the head of the family, who immediately replied that the house was *muito sezonatico* (very aguish), and that he would in consequence be obliged to remove. On my demanding his reasons for considering the house "aguish," "Oh," said he, "every house in Brazil like this,—*muito banhado do vento,*" (literally, much bathed in wind), "is unhealthy, and especially subject to *sezoes,*' (ague), —an opinion which I found fully supported by the popular voice, and which occurred to me as affording the only satisfactory explanation of the cases of ague in my own hospital, as well as of numerous others of a similar nature.

On taking, then, a general view of the circumstances connected with the numerous cases of intermittent fever which had so unaccountably occurred to convalescents in my hospital and in private practice, the conviction was forced upon me in a manner altogether irresistible, that these cases *did not arise from marsh poison, but sprung directly from imprudent or too early exposure to a strong current of cool sea-air* loaded with moisture, while the system was under the debilitating influence of previous exhaustion or disease. The capillary circulation is thus deranged through the impaired energy of the nervous system, and the molecular and vital changes between the blood and tissues are impeded; circumstances of paramount weight in inducing diseased action, and which the more readily arise from the high degree of morbid sensibility acquired by the cutaneous system in tropical climates.

I may here state that I had already observed several most obstinate cases of ague apparently originate in the sudden influence of the depressing passions, at a time when the system had been weakened by profuse and incessant perspiration, and consequently rendered susceptible of the slightest atmospheric or *moral* vicissitude.

Having adopted the above view of the subject, I proceeded at once to act upon it, and to test its truth by the unerring criterion of experience.

Prophylactic Measures.—In the first place, I made several alterations in the wards of the hospital, and had those windows nailed up *which admitted directly* the strong current of the sea-breeze. At the same time, effectual measures were taken to prevent convalescents from being exposed without sufficient warm clothing, or until they had regained a certain degree of strength. The result of these measures was immediate, and the evidence they offered was complete, and apparently free from all fallacy. From that time, intermittent fever almost completely disappeared from among the convalescents in the British Hospital.

The adoption of similar precautionary measures in my private practice was followed by equally striking and conclusive results. The sudden attacks of ague, which had formerly been so frequent among my convalescent patients, and which had caused me so much perplexity and embarrassment. ceased altogether, or rather became of rare occurrence.

In my own case, too, the result was no less conclusive, After having had my health broken up by an ague of fifteen months' duration, and having had the disease arrested by my journey to the Organ Mountains; and although I have subsequently been long and often exposed to the ordinary exciting causes, yet, by carefully guarding against exhaustion of the general system, and derangement of the cutaneous circulation, I remained free from a recurrence of this disease, so liable to return, throughout my subsequent residence of twenty years in Bahia.

DESCRIPTION OF BOMFIM.

Bomfin adjoins the city of Bahia to the northward. It may be described as extending about a mile and a-half from the *Agua de Meninas*, along a broad and well-paved causeway, passing through the centre of an extensive morass, and terminated by a gentle eminence, on which stands the beautiful and, in Brazil, celebrated chapel of "Nossa Senhora do Bomfim." Along this causeway is interspersed an almost uninterrupted succession of houses and gardens, with patches of cultivated ground, gained from the wide-spreading morass which surround them.

The morass, in the midst of which stands the populous suburb of Bomfim, is bounded to the west by the Bay of Bahia, and to the northward and eastward, or *windward*, by a high ridge of hills, which form an imperfect semicircle, separating the morass from the South Atlantic Ocean. It is partially subject to the influence of the tide, and consequently exposed to those deleterious effects which are believed to result from the intermixture of salt and fresh water, together with the immense quantities of vegetable and animal matter, exuviæ, &c., constantly acted on by the powerful influence of a tropical sun.

In the course of my somewhat eventful professional career, I have visited the malarious localities of the Peninsula, and the more deadly shores of Africa and the West Indies; I have bivouacked with the British army amidst the dreary swamps of Louisiana, and have traversed the more classic Pontine marshes near Rome; and I can safely assure you, that in none of these do the elements commonly deemed necessary for affording to marsh effluvia their most concentrated and deadly degree of intensity, so plentifully abound as in the district of *Nossa Senhora do Bomfim.* Yet what is the character of this pestiferous-looking swamp in regard to public health? That it is uninhabitable? Just the reverse. The suburb of Bomfim enjoys the reputation of being, and is in reality, at certain seasons, one of the most healthy districts in Brazil. The higher classes of the inhabitants of Bahia resort to it for the purpose of sea-bathing, during the four hottest months of the year,—*December*, *January*, *February*, *and March.* These are the months when the sun, nearly vertical, exerts its greatest power,—when the swamp is partially dried up,—but still a swamp; exposed to the ebb and flow of the tide, and superabounding, as already stated, in all the elements necessary to the generation of the most abundant and deleterious miasmata. Such is the season at which Bomfim is thronged with visitors; and these, moreover, in accordance with the custom of the country, pass a large portion of the night in the open air, exposed to the influence of the poisonous miasms,—if such there be. Yet, during these four months, a case of intermittent fever is almost unknown; while the towns, Taboao, Rio Vermelho, &c., situated on the sea-coast, and some miles to the northward and eastward,—that is to the windward,—of the hills bounding the Bomfim marsh, and consequently excluded from its influence, but exposed to the full sweep of the humid sea-breeze from the Southern Atlantic, are notoriously subject to ague at all seasons of the year.

How, then, are we to explain the prevalence of ague in towns lying on the sea-beach, and constantly bathed by a powerful sea-breeze; while Bomfim, situated a few miles to *leeward*, is, though placed in the midst of an extensive marsh, wholly exempt from intermittent fever except at certain seasons? The solution, I conceive, will be found in the ridge of hills before alluded to, which breaks the force of the strong humid sea-breeze, tempering, but not excluding it, and thus secures, during the summer season, an equality of temperature rarely met with in other regions of the globe.

Again, the history of Bomfim, during the rainy or winter season, affords the strongest additional evidence in proof of the correctness of the explanation here offered. The wet season commonly sets in about the beginning of April with a sudden change in the monsoon, when atmospheric vicissitudes, considerable for Bahia, often ensue; the rain descends in torrents, and converts the Bomfim marsh into one entire sheet of water; and, above all, the change of wind from about north-north-east to south-south-east at once deprives Bomfim of the protection of that range of hills already so often alluded to, and exposes it to the full force of a powerful monsoon direct from the Southern Ocean. Under such circumstances, agreeably with the received doctrines, all noxious exhalations must be effectually arrested; yet what, in reality, do we now find to occur? We find that Bomfim, having been almost perfectly free from intermittent fever during the summer months, when it *ought* to have been absolutely uninhabitable, now becomes subject to that disease. How, then, are we to explain an anomaly so striking and so inconsistent with the ordinary doctrine regarding the origin or production of ague? The following, I apprehend, will afford the correct solution.

In addition to the large amount of evaporation from such an extensive expanse of water, the powerful south-east monsoon, loaded with moisture, but necessarily free from any miasmatous contamination, now sweeps in, without the slightest impediment, direct from the Atlantic; and Bomfim is thus placed under precisely similar circumstances to those of Taboao and Rio Vermelho, the towns on the sea-coast above alluded to; and the results, as regards the health of the inhabitants, are precisely similar, and continue until the change of the monsoon to the north-east, in October, again restores to Bomfim its former hilly screen, and, at the same time, its wonted salubrity. The few permanent residents, chiefly of the lower classes, who continue to inhabit this district throughout the year, present the ordinary characteristics, though not strongly developed, of the dwellers in low and humid localities.

It is especially worthy of remark, that, along the summit of the semicircle of hills, already described as bounding the marsh to windward, there are numerous habitations entirely unsheltered, and constantly swept by a powerful sea-breeze direct from the ocean. *All* these, without exception, are greatly subject to ague at *all* seasons of the year; while, as already stated, the houses placed below, in the centre of the swamp, are only affected at certain seasons, these seasons being precisely those during which the production of marsh exhalations must be deemed entirely suspended.

In addition to an equable temperature, I deem the uninterrupted prevalence of moderate breezes, by which a stagnant or even calm state of the atmosphere is prevented, to be of the very highest importance in obviating disease in all warm, low, and humid localities. I am indeed satisfied, that were the district of Bomfim subject to the calms and vicissitudes which I have witnessed on the coast of Africa, in the south of Europe, and in America, it would prove not less destructive to human life than the charnel-houses of the Coast and the West Indies.

What impression the proofs adduced in reference to Bomfim may produce on the minds of others I cannot tell. In the statements submitted, from which all exaggeration is carefully excluded, I can detect none of those fallacies by which medical evidence is too often vitiated, nor can I resist the conviction that these statements, even alone, and unsupported by any other testimony, afford irresistible proof that the ordinary doctrines relating to the production and influence of marsh poison are founded in error. Numerous striking and analogous instances are indeed admitted by all. America, the West Indies, Italy, Sicily, are pregnant with unquestionable examples of the security afforded against intermittent fever by woods, hills, walls, &c. *to windward*. The explanation offered is, that they intercept or attract the poison. But how are we to reconcile this with another statement which the supporters of the doctrine of miasm advance with equal confidence, that, while a wood, a wall, or a tree, will at times effectually arrest the progress of marsh poison, yet the same identical poison will, on other occasions, and under precisely similar circumstances, ascend vertically for hundreds of feet to the summit of the highest hills, and there display its deadly properties unchanged!

Again, we are told, that, in some cities, malaria exists only in the low, damp, and crowded quarters of the poor; in others, as, for example, in modern Rome, the close, filthy, and crowded habitations of the Jews, adjoining the banks of the Tiber, are comparatively healthy; whilst the wide, open streets and squares, swept by the chilling tramontane winds, are notoriously subject to disease. To any one, however, who has experienced, as I have at Rome, the difference in temperature between one street and another, and even between different sides of the same street, this seeming paradox will admit of rational explanation, without any necessity for the assumption of a special poison. But I have already, in a former lecture, entered fully into the consideration of these subjects.

In reference to that *vexata quæstio*, the comparative healthiness of ancient and of modern Rome, I am convinced, from personal feeling and observation, that, among other considerations, in a climate subject to such extreme and sudden vicissitudes, due weight has never yet been accorded to the powerful influence of his woollen clothing in protecting the ancient Roman from those diseases so generally attributed by the moderns to malaria.

The Victoria Suburb.—Let us now direct our attention to some interesting and remarkable facts arising out of the political convulsions of the times in Brazil, which afford strong evidence in confirmation of the opinion that intermittent fever does not owe its origin to marsh miasmata.

In the immediate neighbourhood of Bahia, to the southward, within about a mile of the city, stands the suburb of the *Victoria*, where most of the British and foreign merchants reside. Nothing more beautiful than this suburb can well be imagined. It extends about a mile, from the *Campo da Victoria* to the *Graca*, along an elevated ridge of about six hundred feet in height, and overhanging the magnificent bay. The houses are well built, spacious, and elegant, placed on the brow and summit of the ridge, and surrounded by gardens abounding in all the luxuriance of tropical vegetation. Parallel to this ridge, and at the distance of eight or nine hundred yards to windward, stood a thick and lofty woody belt, the "Graca Wood," extending in the direct course of the trade wind, and breaking its force before it reached the Victoria. During the war of independence in 1822–23, when the city of Bahia was besieged, or rather blockaded, by the Brazilians, the suburb of the Victoria was taken possession of by the Portuguese troops and occupied as an outpost; and in order to prevent the advance of the Brazilian forces under cover of the wood, it was entirely cut down by order of the Portuguese general, Madeira. Since that period, the strong sea-breeze being no longer broken by the woody screen, intermittent fevers have been of much more frequent occurrence in the Victoria. Furthermore, in numerous instances, particular houses have suffered from similar causes;—namely, the cutting down, by order of the Portuguese general, of a tree, or the destruction of some wall or building which had previously afforded shelter from the strong humid sea-breezes.

From a like cause, houses in elevated and exposed situations are subject to ague in Brazil, though less so when built of one storey, and surrounded by a deep verandah, which breaks the force of the breeze, than when the ordinary height and construction are adopted. Instances of this nature are numerous and well marked. So well understood and universally established are the above facts in Brazil, that no native Brazilian will build his house on an eminence, exposed, without protection, to the full influence of a strong sea-breeze.

Conversely, it is matter of common observation, that many houses in exposed and elevated situations, or built on the gorge of hills, and thus subject to a strong current of cool, damp sea-air, have, though formerly almost uninhabitable from ague, become healthy since the growth of forest timber in the immediate neighbourhood, *to windward*, which had evidently no other effect than that of interrupting the force of the strong, cool, and damp, though otherwise healthy wind.

In estimating the influence of a powerful cool and humid wind on the animal economy under the foregoing circumstances, we must always bear in mind that the cutaneous system is continually relaxed, and the nervous power depressed, in tropical regions; that the difference in temperature between the sun and the shade is very great; that the slightest bodily exertion is attended by strong vascular excitement and profuse perspiration; and though, during the continuance of such exertion, little or no danger may be apprehended, that its discontinuance is usually followed by great exhaustion, and, to a certain extent, by collapse. Hence arises the paramount importance of well-sheltered situations, and the danger, in such climates, of exposure to strong damp currents of air in certain states of the system.

On these principles, too, I apprehend, we can satisfactorily explain the peculiar and unlooked-for unhealthiness of certain barracks and stations, placed in elevated and exposed situations in the West Indies, rather than by imagining the ascent of a special morbid poison from the lower and surrounding district, whose inhabitants it leaves comparatively unscathed, to display its deadly influence, notwithstanding an extreme dilution, on the residents of the distant ridge or mountain.

The particular state of the animal economy just alluded to is well understood by the native Brazilians, and guarded against most carefully as a chief source of *sezoens* (intermittent fever) as well as of *constipaçoens* (continued fever). After exercise, or long-continued exposure to the sun, they invariably shut themselves up for a time in a close room; and though of temperate habits as regards spirituous liquors, they take some stimulant in order to guard against the dangerous effects of the sudden collapse, which so frequently ensues.

The influence of sudden change from extreme heat to comparative cold, under certain conditions of the system, in exciting an attack of a febrile type, was well exemplified in my own person, while travelling in Italy during 1839, for the benefit of my health. On 1st July, I quitted Rome for Naples, in company with my friend Mr Edwards, like myself an old resident in Brazil. At two o'clock, P.M., the thermometer stood at 87·5° in the shade, in the Piazza de Spagna, and the heat was much more oppressive than I had ever experienced in the tropics. We set off in the afternoon, preferring the danger of a night journey over the Pontine

Marshes to the suffocating heat of a mid-day sun. Some time after leaving Albano, a spring of our carriage gave way, which compelled us to walk nearly a league to Gensano, where we arrived shortly after midnight. Here we found the inhabitants in such alarm, from a recent appearance of brigands in the neighbourhood, that they refused to open their doors, and we were compelled to remain in the open street, whilst our attendant patched up the damaged carriage, which occupied upwards of an hour. Never shall I forget the intense chilliness which I experienced in the streets of Gensano, in my summer clothing, fatigued by the excessive heat of the previous day, and bodily and mentally exhausted in taking a last look at the "lone mother of dead empires." My teeth almost chattered with cold; and on reaching Velletro, at three o'clock in the morning, I had fairly entered on the first stage of an ague fit, with visions of malaria floating in my imagination. Here, however, I managed to obtain some hot coffee, together with a supply of bad brandy, which had the effect of restoring the nervous power, and setting my blood once more in circulation; and thus I was enabled to pass the dreaded Pontine Marshes without further inconvenience. On reaching Terracina, at nine o'clock in the morning, the bright shores of the Mediterranean, aided by the less imaginative, though probably not less important influence of a good breakfast, quickly dissipated all recollection of marshes and malaria.

Now I am perfectly convinced that, had I not obtained the hot coffee and-brandy at Velletro, I must—marsh poison apart—have suffered an attack of fever; the type, severity, and duration of which would have been determined rather by constitution or predisposition, than by anything specific in the original exciting cause. This exciting cause I take to have been exposure to the cold damp night air, at a time when mind and body were alike suffering from previous exhaustion. What the actual thermometrical difference might have been between Rome at three P.M. and Gensano after midnight, I cannot possibly determine; but were I to measure it by my own feelings, I should certainly state the difference to not less than 40°; and if we take into consideration an atmosphere loaded with moisture from the adjoining marshes, a clear and cloudless sky, and the high radiating power of the rank luxuriant grasses, I am satisfied that the estimate of 40° would not be far from the truth. A change so great and so sudden, when applied to an over-excited and exhausted system, will satisfactorily account for the invasion of fever, without calling in the aid of an especial poison.

There is certainly nothing intrinsic in the essential nature of intermittent fever, which absolutely precludes the idea of its arising from other causes than the reputed one of a special poison.

The question of periodicity, considered by the mass of medical authorities as almost distinctive of diseases originating in marsh poison, I have already disposed of. I have moreover shewn you, that the disease in question has been brought on simply by periodical immersion in cold water; and a singular case has been recently reported of one of those unfortunate outcasts of humanity, who prey on the feelings of the public, through the habit of feigning shivering, and who is now stated to be unable to bring his body into proper warmth—he always shivers.

Let us examine the common history of a man who has suffered from an attack of ague. He is often liable, after the lapse of weeks, or months, or even of years, to a repetition of his attack on exposure to cold, or wet, or an east wind, &c. Now, what are the explanations of this remarkable phenomenon ordinarily offered by authors? One deems the law of habit to afford a satisfactory solution; another believes that the malarious poison is still lurking in the system; whilst a third, as Dr Elliotson—high authority—supposes that the unhappy wight, in passing through a market-place, may have come in contact with some erratic and malarious vegetable; and thus he satisfactorily accounts for the renewal of the disease.

With regard to the law of habit, I apprehend that, whatever may have been its original force, an interval of months, or years, must in all cases be deemed sufficient to annihilate its power, to say nothing of its first establishment. I also hold, as utterly untenable on chemical evidence, the doctrine of a poison remaining unaltered for years in the system, though exposed to the incessant series of organic changes and decompositions to which the human frame is subject, as well as liable to the universal law, inherent in all organised matter, of perishing, or dissolving into other forms. And the question of the malarious influence of decaying vegetable matter in the common highways and byways, will be, I conceive, sufficiently set at rest by the well known-fact, that Covent Garden is neither a pest-house nor a desert.

It would be important to know whether the authorities, to whose opinions I have just alluded, have ever carefully investigated the actual condition of health in these individuals who are subject to occasional attacks of ague. My own peculiar position has enabled me to make such examinations on an extensive scale; and the following are the results to which I have been led. In every case of relapse which admitted of satisfactory investigation, I found, universally, that though the first attack of ague had been subdued, and the patient restored, in common phrase, to his ordinary health and his ordinary occupations, yet his health had not in reality been perfectly re-established. If questioned under such circumstances, the reply would be that he felt perfectly well; but on a more close examination, it would be found that he had become more sensitive to slight atmospheric changes; that his tongue was rather white and flabby, his appetite capricious, his feelings often causelessly depressed; that he was less capable of supporting continued mental or bodily exertion; and that his urine especially had become subject to much variation, being occasionally pale and rather abundant, and at other times scanty, and depositing sediments. Still he would be considered in good health. This condition of system may, under favourable circumstances, continue for a considerable period with little change, and finally pass away altogether; or the individual being accidentally exposed, in the above state of the economy, to one or other of the usual cases of fever, as cold, wet, and east wind, &c., a fresh paroxysm of ague may be the immediate consequence.

In this mode, according to my experience, is to be explained that peculiar susceptibility to relapse in ague, which has so long and so seriously taxed the ingenuity of authors to account for satisfactorily. The influence of change of scene and climate in these cases is well known, and, in the higher latitudes especially, often affords our only resource. The mode in which this change operates so beneficially in ague, has also given rise to various conjectures and opinions. When we reflect, however, on the remarkable power exercised by change of climate over the nervous system, and on most chronic maladies unattended by organic change, we need surely resort to no very recondite hypothesis, but may at once admit the obvious fact, that the general condition of the economy had been so far modified and improved by change of scene and climate as to be unable to resist those ordinary exciting causes of disease to which it had previously succumbed.

I quote the following extract from one of the most recent works on the subject, as it suggests more correct views of the elements of febrile diseases in tropical regions; and the principles of treatment glanced at are unquestionably superior to the fatal routine system of venesection, calomel, and salines, so commonly pursued in these maladies. The author, however, seems unacquainted with the almost miraculous effects of quinine in large doses, in controlling every stage and form of the intermittent and remittent fevers of hot climates.

"European practitioners, in any degree conversant with the medical customs of the negroes of intertropical Africa, cannot fail to be deeply impressed with the marked attention paid by the native doctors to the due action of the cutaneous tissues, and their encouragement of this as a means for relieving disease. The Mahometan code of laws, whose sanitary regulations are so well adapted for the advancement of the moral and physical condition of the barbarous pagan tribes in Central Africa, strictly enjoins not only abluent, but other hygienic measures, for the promotion of cleanliness, and the proper discharge of the cutaneous functions. The inhabitants of most of the maritime localities in the Bights are fully acquainted with the importance of these views, and treat the remittent, and other fevers to which they are subject, by endeavouring to excite a long-continued and copious exudation of sweat from the cuticular pores, by the aid of heated sand-baths, ablutions of hot water, and rude attempts to imitate vapour baths. In some countries, the patient is placed to a large fire for such purposes; while, in others, he is held over it, water being slowly dropped thereon, so that the steam, as it ascends, may act on the affected portion of the body. After a careful observation of the good effects of this remedial system, I was led to pay more particular study to the utility of its application, and, at length, to try a modified adaptation of it for the cure of those adynamic remittent fevers so destructive to Europeans. I have no hesitation in saying, that not only myself, but many others, who have experienced its efficacy by the speedy restoration to health, can vouch for its superiority over the ordinary practice of venesection, saline purgatives, and large doses of calomel," &c.—(Pp. 119–20.)*

It is scarcely necessary to repeat that, notwithstanding the almost universal assumption of the hypothesis of malaria, no sufficient evidence has yet been adduced to prove its *entity*. Indeed, so far as all chemical investigations go, the most refined analysis has been altogether unable to discover the slightest difference between the air of the most swampy fen and air procured from the most pure and open sources. The actual presence of malaria is inferred from the succession of certain effects; but the question has never yet been fairly raised, whether certain *known* conditions and states of the atmosphere, and of the individual, might not be deemed capable of explaining the phenomena more satisfactorily than the agency of a cause which is purely hypothetical, and which has never yet been brought to the test of experience.

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* Daniel, W. F., M.D. Sketches of the Medical Topography and Native Diseases of the Gulf of Guinea, Western Africa. London, 1849.

I humbly submit that this question may be satisfactorily answered in the affirmative, on the evidence which I have adduced, and which at least demonstrates that intermittent fever does originate in numerous instances, and probably universally, from certain atmospheric conditions in certain states of the constitution, altogether independent of organic or marsh poison.

Unity of Fever.—The whole group of febrile diseases, the essential meaning of this term being a disturbance in the sanguiferous system, may be comprised in three distinct classes,—fever without local action, fever with local action, and fever with a specific action, and arising from a specific cause.

The connection of the second class or group with atmospheric vicissitudes, is too palpable to demand illustration. The dependence of the last on a special cause, altogether apart from the physical influence of the elements, separates it distinctly from the others, so far as regards the cause. But the origin of the first class is undoubtedly, in numerous instances, as frequently the result of atmospheric vicissitudes as of any of the other commonly accredited causes, contagion or malaria. In all the three classes, the common feature of family resemblance is portrayed in the initial symptoms,—the nervous depression, of longer or shorter duration; the cold shivering and anserine skin, followed by the glowing heat and general perspiration.

It is, indeed, more than probable, that fever is the expression of a type of disease essentially one and uniform, but admitting of an almost endless variety of forms, the simplest being displayed in the single paroxysm of an ague: It is furthermore obvious, on the most cursory observation, that the description of fever as a disease has, in the generality of instances, been drawn for certain localities, and not from the whole group of febrile diseases, as witnessed in different parts of the world. The typhus fever of this country is superseded by the bilious remittent and intermittent in southern climates, by the plague in the Levant, and by the yellow fever within the tropics. Each of these maladies, under the special influence of climate, temperament, different modes of living, and numerous other agencies, affects certain peculiarities in its progress; but they are all distinctly impressed by the phenomena universally characteristic of fever as a genus of disease in every clime.

Climate, indeed, and its varieties, whether permanent or temporary, always modify the animal economy, and induce, if I may be allowed the expression, an epidemic or endemic state of the human constitution exposed to its influence. This state will vary, as the atmosphere may be cold or hot, dry or humid, pure or impure, variable or equable, stagnant or the reverse; the geological character of the country or district, also, and the numerous moral and physical conditions already alluded to, powerfully contribute towards the formation of this endemic constitution in man. Moreover, the history of epidemic and endemic diseases almost universally proves that, prior to their invasion, the country or district has been subjected, for a period more or less prolonged, to some well-marked deviation from the ordinary constitution of the climate, sufficient to modify the constitution of the inhabitants generally; so that the slightest disturbing cause, moral or physical, is followed by disease, the characteristics of which are determined as much by the constitution of the individual as by the constitution of the atmosphere.

An extensive acquaintance with the fevers of different regions of the globe has gradually established in my mind a conviction of their essential identity. And this has been confirmed since my return to Europe in 1842, by my observation of the typhus fever of this country and of France, which, in its more intense form, differs rather in degree than in essence from the remittent of hot climates, or the yellow fever, as I observed it during the epidemic of 1812–13 at Cadiz, while serving as resident medical officer at the General Hospital for the reception of the sick of the army of Andalusia. In each you have commonly the same tendency to remission; and there is no one symptom, even to the yellow skin and black vomit, which you will not find occasionally present in each of the three diseases; neither is there any one organ or system so constantly affected, nor any lesion so constantly detected on *post-mortem* examination, as to characterise the several maladies. There is no one morbid result, in fact, in any of these forms, that you will not occasionally find in all.

The commonly rapid course of the tropical fevers might, perhaps, be urged as sufficient to distinguish them, and to establish their claim to be placed in a distinct category: but do not other tropical diseases often run an equally rapid course as compared with those of Europe, and yet who ever dreams of their being essentially different from similar maladies in colder latitudes?

The remittent, or bilious remittent, of southern climates, I need scarcely observe, is simply a more intense form of intermittent, and differs only in the severity of the symptoms, the less perfect remissions (intermission really applies to neither), and the greater disturbance of the organic functions. They are to be treated on the same principles, and are controlled, fortunately, by the same remedies.

In reference to that *vexata quæstio*—the essential identity of the bilious remittent with the yellow fever of hot climates—I may observe that the latter disease has lately ravaged the coast of Brazil, it is said for the first time, and has therefore given rise to much difference of opinion among the profession. Some maintain its foreign, others, as my friend Dr Croker Pennell, of Rio de Janeiro, its indigenous origin. In proof of the latter opinion, Dr Pennell states (p. 19), that "It is a curious circumstance, and may perhaps tend to elucidate the origin of yellow fever in Brazil, without having recourse to a specific source of infection, that for the last few years, the fevers of the country, evidently not infectious, but of high temperature or marsh origin, have clearly been changing their characters. The genuine remittent has been but little seen for the last three years. In 1847–48, and '49, it was replaced by a fever of its own class, popularly known by the name of 'Polka,' but in reality a remittent; and, during the present year, *it* (the Polka) has been replaced by the yellow fever, a disease also with similar features.

"Coincident with these and other changes in the diseases of Brazil, the climate, in its broad features, has altered strangely. Thunder-storms, formerly of daily occurrence at a certain hour, during the summer, are now but seldom heard," &c., &c. Dr Pennell finally arrives at the following conclusions.

"That bilious remittent and yellow fever exist only under similar conditions of locality and climate;

"That they are essentially the same disease;

"That yellow fever is the most intense form of bilious remittent:"—propositions entirely in accordance with my own experience in Brazil and other countries.

On the other hand, my friend and successor at the British Hospital in Bahia, Dr Alexander Paterson (whom I am happy to recognise now present), considers the yellow fever, which prevailed at Bahia at the same epoch as in Rio de Janeiro, to be a disease *sui generis*, and imported; and he has published some valuable and interesting evidence in support of this opinion.* Along with other reasons, he infers its essential difference from the ordinary remittent fever of the country from the fact, that quinine, which almost invariably controls the latter disease, proved powerless in the former. This evidence, however, I hold to be inconclusive. In the worst form of yellow fever, *no remedy*, Gentlemen, as I know by experience, will modify the disease—a fact, unfortunately, applicable alike to the worst form of remittent and of intermittent fever. All three may, indeed, be characterised in the terse words of a distinguished French Physician, as applied to cholera—"They commence with death." And, I may add, it is impossible to distinguish, in any thing essential, the more intense form of these tropical fevers from the more intense form of cholera—all commence with death; the patient never for an instant rallies; the rational and physical signs are, in all, perfectly identical; and equally so are the *post-mortem* appearances.

While this sheet was passing through the press, my attention was directed to an interesting article in the last number of the *Dublin Quarterly Journal of Medical Science*, by Mr Barton, on Hong-Kong fever. Mr B. is a believer in the theory that marsh miasm or malaria is the general exciting cause of intermittent and remittent fevers; but, as a candid and accurate observer, he does not resist the clear evidence of facts, and he therefore admits that these diseases *may* arise from other causes than marsh poison. In proof of this he gives his own case. He had resided at Hong-Kong for four years in perfect health, until placed in attendance on the troops, and exposed to "unusual mental and bodily fatigue," when, after one month, he was seized with remittent fever. He however believes (p. 347), "that a poison is imbibed into the system, remaining latent for an uncertain period, or until some exciting cause develops a febrile paroxysm;" though he admits that a combination of depressing moral and physical influences might account for the disease. He considers "that the organic nervous system is primarily affected," in which opinion I fully concur. He notices the "cholera form as simulating Asiatic cholera;" there is, in fact, no distinction.

In proof of malaria being the source of fever, he notices that "the civil population at one time suffered equally with the troops; but subsequently, on proper sanitary steps being adopted, remained comparatively healthy . . . . and the mild character of the disease in the royal navy and persons living afloat."—(P. 348.)

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* Medical Gazette, 28th March 1851.

To this I reply, that were the royal navy deprived of their accustomed accommodation and advantages, or the civil population subjected to the same exposure and privations as the military, we should find that fever respected the one class as little as the other.

He remarks (p. 363) on the fearful amount of mortality amongst the troops, as contrasted with the healthiness of the naval forces and of the civilians; and he very rationally suggests the amelioration of the condition of the soldier as the remedy, adding, "the reputed pestilential nature of the climate is now only supported by the military inhabitants, as other individuals, untrammelled by their rules and regulations, enjoy an equal if not greater amount of health . . . than most other tropical residents;" and as a proof that the disease is not attributable to locality alone, we have the further fact, that "the officers of the 59th regiment, quartered in the same locality as the men, but better provided, having more space allotted to their use, and not being exposed at night, escaped fever."—(P. 334.)

Still Mr Barton believes "that a poison is imbibed into the system," and that "the system is impregnated with this subtle essence, is shewn by the constant liability to relapse." He admits, however, that "suppression of perspiration is a frequent exciting cause, as instanced in the number attacked on or after night guards, from exposure to draughts of air at an open window, and the frequent relapses of the patients unavoidably placed near the doors in the hospital wards."—(P. 344.)

He attributes to the concentration of malaria, or marsh exhalations in certain localities, the rapid putrefaction of dead animal matter, the rapid decay of silk, woollen, and other fabrics, and the ground floors in buildings becoming quickly rotten, &c. &c., and believes these to confirm "the theory of a noxious emanation from the soil."—(P. 345.) These results I have elsewhere shewn to be invariably contingent on the combination of high temperature and moisture, and altogether unconnected with marsh or any other noxious exhalations.

But the evidence adduced by Mr B. himself against the malaria hypothesis I hold as irresistible, especially when conjoined with that afforded by Brazil. Mr B. refers to Dr Graves of Dublin on the question of the origin of fever in

Ireland; and I am well satisfied, that, had the penetrating and philosophic mind of this physician been afforded the opportunity of observing tropical fevers, the marsh poison theory would have long since disappeared.

Mr B. tells us, that "at Stanley or Chuckchoo, a military station at the south side of the island, the barracks are close to the water and open to the sea-breeze." A detachment of troops arrived from England in October, and were quartered here, and "*all* were attacked with fever within a fortnight after their arrival, and at a time when malaria could not be considered as exerting much influence."—(P. 346.)

Again, "the south-west monsoon blowing during the summer months, and the town of Victoria being situated on the north side of the island, and under *the lee* of the highest range of mountains, it was conceived that the exclusion of the refreshing sea-breezes was one cause of sickness. A military station was accordingly formed on the south side, and barracks erected on a *peninsula open to both monsoons*, and enjoying the advantage of having *no swamp or marsh* in its neighbourhood. The mortality here exceeded the worst periods experienced at Victoria on the north shore, and the place has been *virtually abandoned*. . . Six assistant surgeons ordered there during the past season to replace each other in succession, were all attacked with fever and removed to Victoria."—(P. 346.)

"The same cause, whatever it may be, evidently gives rise, indifferently, to remittent fever, acute dysentery, or intermittent fever."—(P. 346.)

Mr B. also suspects (there is no doubt of the fact) that "causes which in a temperate climate give rise to continued fever, or inflammations of the thoracic viscera, in a tropical climate, cause periodic fever, or determination to the abdominal viscera."—(P. 347.) Though holding to the marsh miasm theory, he still thinks "that it is not *altogether improbable* that periodic fever may arise independently of malaria, and be owing to heat or peculiarity of climate remotely, and immediately to sudden chills, or any depressing agency."—(P. 336.)

"The city of Victoria is situated on the northern shore, and stands close to the base of the highest ridge, the buildings extending up the face of the hill. . . . The town enjoys the advantage of being built upon a dry soil," &c. "The Regimental Hospital and Military Barracks are built at the foot of ravines or mountain gorges. . . . The winds during the hot season blowing through these ravines towards the military buildings." "Whatever the cause or causes of the fever may be, one fact is incontestibly established, that the vicinity of ravines and gorges in the mountains is unhealthy. . . . Both the Murray Barracks and Hospital are built at the foot of ravines, and the inmates will, in my opinion, always be obnoxious to attacks of fever. . . . Men admitted for other diseases were attacked with fever. . . . Observation proved that relapses were most frequent in the eastern wards of the hospital, which was opposite a *cutting in the hill* leading from the ravine behind."—(P. 338.)

Dr Wilson's '*Medical Notes on China*" are quoted with reference to one locality which proved singularly fatal. "It is a low narrow gorge where a line of six cottages were lately built, with high land on either side; and close to one extremity of the range there is cultivated rice ground, while there are deserted spaces at the other. From the height of the land forming the gorge, the wind is diverted from its natural course, follows the hollow, and thus blows over the *rice land*, cultivated or waste, to the cottages. Five English gentlemen went to reside there; in a short time four died from fever, the fifth scarcely escaped with life, and the lodging has been abandoned. . . . . Three large and commodious houses were erected, of which two have been razed to the ground by their proprietors as uninhabitable, owing to their insalubrity, ... While the city of Victoria (*sheltered, recollect, by the highest ridge in the island*) enjoys almost entire immunity from fever, and the few cases that do occur are of a mild and tractable nature."—(P. 341.) "The bad cases of fever always found *in exposed positions*, or in the vicinity of ravines or gorges."—(P. 338.) "High temperature night and day for four or five months . . . . and atmosphere very humid."—(P. 343.)

But every page affords an illustration, which I would willingly quote did my space permit; and I am compelled to refer the reader—as I do most earnestly—to the paper itself, which will amply repay perusal.

The explanation, indeed, offered for the unhealthiness of the several buildings erected close to the ravines or mountain gorges, or in the valleys, is the old story of rice lands, decomposing brushwood, rank vegetation, or decayed vegetable matter carried down by the torrent, &c. But what shall we say of the barracks at Stanley—"close to the water, and open to the sea-breeze;" or to the still more deadly, and now abandoned station—"erected upon a peninsula open to both monsoons, and enjoying the advantage of having no swamp or marsh in its neighbourhood;" whilst Victoria, built close to the base (*to leeward*) of the highest ridge on the island, is free from fever!

Let the reader compare with the above statements the evidence afforded by the city of Bahia, the Bahia Hospital, St Lazaro, Bomfim, &c., and he will at once perceive the solution to all Mr B.'s doubts and difficulties at Hong-Kong. Mr B. adopts the universal theory of malaria or marsh poison, and therefore considers that the vegetable products of the adjoining ravine afford a satisfactory explanation of the deadly insalubrity of his hospital and barracks;. whilst Ferguson, in another hemisphere, found the Rocky Mountain gorge, devoid alike of vegetation and moisture, equally fatal, and from precisely similar diseases; and he, too, invoked a malaria! How, then, are we to reconcile such seeming anomalies? Thus: in all hot climates (from causes stated elsewhere), if your hospital, or barrack, or habitation, be placed on an unsheltered eminence, or in the current of a gorge or ravine, though such ravine, instead of decaying vegetation, shall be lined with Parian marble, and swept by the purest ocean winds—pure, but always loaded with moisture—yet will such hospital, barrack, or habitation, prove invariably unhealthy; and ague, remittent, and continued fever, dysentery, and rheumatism, the prevailing diseases.

From the height and pride of our theories, might we not cast a glance at '*ces bons Chinois*,' who ought surely to know something of their native country, and who are, besides, "subject to periodic fevers.'' They select *invariably* sheltered situations for their detached houses and villages, both being surrounded by *umbrageous trees* and shrubs'' (p. 344), precisely as the Brazilians do under similar circumstances, in another quarter of the globe, and doubtless for the same substantial reasons.

Let us then be guided by rational principles; let us place our habitation or our hospital in some dry and *well-sheltered* locality, and avoid alike the exposed summit of the hill, or the cool and grateful, but deadly blast from the gorge or the ravine. At Hong-Kong, as everywhere else, let the comfort of the soldier be equally cared for as the comfort of the civilian; let him be supplied with proper food, appropriate clothing, and commodious barracks; let over-fatigue, exposure to the mid-day sun, and night-duties, be, as far as practicable, lessened or avoided; let temperance, rational amusement, and healthy exercise be encouraged; as well as all other means calculated to maintain the moral and physical energies; then drain, plant, and cultivate, by all means;—by which we shall not only increase shade and shelter, and lessen moisture, but add otherwise to our personal enjoyments.

I need not here repeat, what will be fully explained elsewhere, that to the observance or neglect of the above measures, and not to visionary and occult influences, we must look for the maintenance of the health of our troops and stations in all intertropical countries. We must, in short, cease to be terrified by shadows, and attend to realities.

I greatly regret that space will not permit me to enlarge on the important questions of symptoms and treatment. I would, however, observe, that the greater gravity of the fevers, generally at Hong Kong, as compared with those of Bahia, is explained chiefly by one single fact, viz., at Hong-Kong, the thermometer ranges from 45° to 91°; at Bahia, from 72° to 86°.

One or two more observations, and I conclude. Quinine was found *the remedy* at Hong-Kong; yet, prior to Dr Ferguson's arrival, " his predecessors reported quinine to have *signally failed*."—(P. 356.) " Bleeding and calomel was tried with almost uniform ill success; . . . . depletion and antimonials were equally unsuccessful."—(P. 357.) " One day's observation of the disease in the Military Hospital was sufficient to demonstrate that quinine in *small doses* was inadequate," &c. " The freedom from visceral disease of those cases treated with quinine at an early stage was marked." —(P. 357.) ' Thirty to thirty-six grains commonly induced the specific action of quinine on the nervous system;" and " in but one case out of nearly 900 subjected to its influence, were any bad consequences experienced." These were " sudden sinking or collapse" (p. 355), which speedily passed off on using stimulants.

My experience, in some points of treatment, does not coincide with that of Mr Barton; but into this subject I am unable to enter.

Art. VI.—*Facts and Observations, on a Particular Form of Mental Disorder, in relation to the Mental Condition of M. B.* By M. Brierre de Boismont, Director of an Establishment for Insane Persons; and M. Boys de Loury, Surgeon to the House of Saint Lazarus. — (*Annales d'Hygiène*, t. 44, July 1850, p. 414.)

The difficulty of defining insanity, in a manner perfectly free from objection, is well known to all those who have seen much of the disease; and there is not a more certain method of involving a medical witness of limited experience in contradictions and absurdities, than by requesting him to give a definition of Mental Derangement. One witness gives a definition in one set of terms, which are found upon trial to exclude several examples of mental derangement, which if not well marked, are at least not of unfrequent occurrence. Another gives a definition which would include, not the inmates of asylums so much, as a large proportion of those who, in ordinary society, are daily observed to conduct their affairs as well as the generality of mankind do. We have seen a physician to a lunatic asylum, define insanity to be a want of the average amount of intellect. It is, on the one hand, difficult to say, what amount of intellect is the average; and it is certain that a large number of persons who are supposed by their neighbours to be what is called below par in intellect, succeed in passing through life in a creditable and useful manner, supporting themselves and families, and not unfrequently leaving comfortable fortunes, acquired by industry and economy. It is, on the other hand, not less certain, that not a few persons, whose intellects were allowed to be much stronger, or much higher, or much more comprehensive than the generality of their fellow-creatures, have at certain periods of their life shewn that they were the subjects of trains of thought and modes of thinking, which the rest of mankind are disposed to regard as indicative of mental derangement.

1. It has been several times stated, that Sir Isaac Newton was, at one time of his life, in a condition which, in any other person, would have been regarded as mental disorder, and which many persons consider to have been distinct, decided, and unambiguous derangement.

It has been stated, for instance, by Biot, in his *Eloge*, or Life of Newton, that this great mathematician and profound reasoner was, in the year 1693, in a state of mental aberration; and that it was only after proper care had been taken of him for several months, that he recovered his faculties so far as to be able to understand his own PRINCIPIA. To this statement, and the occurrences which gave rise to it, much importance has been attached. Sir David Brewster, however, has shewn, that only doubtfully can it be said that symptoms of derangement had actually taken place. All that is certain is, that about the end of 1692 or beginning of 1693, his chemical laboratory had been destroyed by fire, and a number of valuable manuscripts, containing calculations of great labour, had been burned; and not long after several manuscripts had been consumed by a candle, which he had left burning, when he went to chapel. A statement from the journal of a contemporary student at Cambridge, is to the effect, that when Mr Newton returned and saw the damage done, every one thought he would have gone mad; and he was so troubled at the loss, that he was not himself for a month after.*

It is thus merely an inference of others, that Isaac Newton might have gone mad, not exactly that he was mad.

We have nevertheless heard an eminent mathematician declare, that when Newton wrote his Commentary on the Apocalypse, his intellect was in a state of confusion and disorder. It is to be observed, however, that this gentleman drew the inference from the fact of his writing a Commentary on the Apocalypse, and not from any collateral evidence.

It would scarcely be correct, therefore, to conclude, from the uncertain facts now mentioned, that the mind of Isaac Newton was actually in a state of derangement; and this case, therefore, cannot be received as any evidence, either in the one way or the other.

There are, nevertheless, many examples of persons whose intellect was clear and powerful, and whose general mental energy was much greater than falls to the lot of mankind generally, becoming, at certain periods of life, under peculiar circumstances, the temporary or permanent subjects of mental derangement. Without attempting a view of all these instances, let us shortly advert to a few of the most remarkable.

2. Torquato Tasso, if not the first of Italian poets, holds a high rank among that body, for genius, invention, descriptive powers, and all that constitute the great poet. Among his countrymen he is inferior only to Dante; and if he be below Virgil among the Romans, and Milton among English poets, it is only because these two have risen so high above all others. Tasso, therefore, had intellect far above common. Fancy and invention he possessed in an eminent degree. Sublimity of conception, with great knowledge of the human heart, with unrivalled power over his native language, it is impossible to deny him, Yet this man, so nobly endowed, was for a great part of his life, in a state of deplorable mental derangement. His whole thoughts were in a state of confusion and disorder. His moral qualities were subverted. Over his actions, judgment had no control.

The circumstances of the life and mental disease of Tasso are well known, and have more than once been the subject of discussion. His derangement, which assumed the character of what is called *Lypemania*, or melancholy madness, was in some sense hereditary. His mother died at an early age in violent delirium. His father, whose life was a long scene of distress and adversity, became latterly melancholy and despondent. The loss of his mother, and the calamities of his father, are said to have operated on the mind of the son, as soon as he began to think. At an early period, indeed, Tasso was subjected to treatment which was well calculated to spoil and turn an ordinary head. His father, Bernardo Tasso, had come to the court of Pesaro in 1556; and six months afterwards he was joined by his son Torquato, who was permitted to share the education of the hereditary prince, and to mingle occasionally in the elevated circle at the Imperiale, until his father carried him to Venice in 1559. At the University of Padua, whither he was sent to study law, he is said to have written, in his eighteenth year, the poem of *Rinaldo* in the space of ten months. This was published in 1564. In 1563, we are informed, he was at Bologna, professedly pursuing the study of the law, but in reality, as at Padua, spending his time in writing verses; and here, it is said, he began his *Gerusalemme Liberata.*

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* Life of Sir Isaac Newton, by Sir David Brewster.

At the age of twenty-one, that is, in 1564, he was introduced at the court of Ferrara, an incident which certainly sorely tried whatever judgment he possessed. It is of little or no moment, whether he entertained hopes of exciting an interest in the breast of the Princess Lucrezia D'Este, or her youngest sister, Leonora D'Este, which it is said he attempted in 1571, upon the marriage of the former. Either or both events shew that he was a man of a very weak or a very rash mind. To the dispassionate and elevated spirits of northern countries, there seems something inexpressibly mean and foolish in the constant effusion of extravagant poetical compliments offered to these ladies; and certainly not the beauty, either of the thought or language, can rescue the giver from the character of being a vain, weak man, who prostituted and degraded his mental talents. There is, indeed, in the character of Tasso a strange combination of opposite qualities. In 1571 he appears to have visited France; and when he returned in 1572, he was, by the intercession of the Princess D'Este, received as a salaried courtier at Ferrara. The following spring, his pastoral drama, *The Aminta,* was performed at the palace; and the applause which it received led to the Princess of Urbino inviting him to Pesaro, whence he accompanied Francisco Mario to Castel Durante. In 1574 he returned to Ferrara, loaded, it is said, with compliments and presents.

At the age of thirty, his health is stated to have been irremediably broken by attacks of intermittent fever, upon a constitution originally not strong. Independent of this cause, however, he seems to have spent his days in a state of constant excitement and uncertainty as to prospects and means of assured independent subsistence. His mode of life was, indeed, calculated to improve neither the energy of his mind, nor the strength of his frame. A dependent in the court of Urbino, without any prospect of support, except that resulting from the cultivation of an agreeable and beautiful talent, though not one absolutely necessary, the consciousness of his precarious mode of subsistence, united with a proud haughty spirit, prone to take offence, and suspicious of all around him, tended in no long time to unsettle his reason, and make him the victim of ungovernable passion.

In January 1576, when he returned to Ferrara, being then in the thirty-third year of his age, he was, on the death of Pigna, appointed historiographer to the reigning family. This event might have been expected to have rendered him at once more independent in spirit, and more steady. It did not. An affront, either slight or imaginary, from one of the courtiers of Ferrara, first involved him in a duel; and after this he evinced nothing but the restlessness and apprehensions of the lypemaniacal lunatic.

While seated in the apartment of the Duchess of Urbino in her mother's palace, in June 1577, he rushed with his dagger on an attendant who happened to enter. Whether this was a premeditated assault, or the result of an illusion or hallucination, is not known, and probably would be idle to inquire. Dr Andrew Verga, the most recent inquirer, is induced to ascribe the attack to hallucination.* It is evident that to search for reasons in the acts of Tasso at this time implies that he was sane; but all other circumstances tend to shew that he could not, or did not control his actions. He was placed under restraint; and though set at liberty upon the subsidence of the paroxysm, he was convinced that he was in disgrace, and was persecuted by secret enemies. Tasso then left the court, and entered a Franciscan convent for some time. Here, however, the illusions continued. He again changed his residence to Sorrento; and again after a few months returned to Ferrara. Here the mental disorder shewed itself in distinct and threatening symptoms; and he was placed, on the 21st February 1579, in the hospital of Santa Anna. Here seven years were passed with little amendment. Fever is said to have attacked him; probably it was merely an acute form of the cerebral disorder. Upon the abatement of the symptoms he was removed from the hospital and committed to the care of Prince Vincenzo of Mantua. Life was prolonged nine years, spent partly at Naples; and in 1594 he went to Rome, where he retired to the Monastery of Santo Onofrio. Here in 1595 he is said to have caught malaria fever. He died on the 25th April 1595, at the age of fifty-two years.

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* Bibliotheca Italiana, No. XXXI. Milano, Maio 1845.

It is impossible to place upon the events and transactions of this life any other construction than that of mental derangement for the last nineteen or twenty years. There are the restlessness, the suspicion, the unfounded apprehensions of the disordered brain. The letters of Tasso during the time specified, shew clearly the unsettled thoughts of a person whose mind is obscured, and whose mental faculties are in a state of confusion. The judgment was upon certain points wrong, and never could be rectified. The intellect, nevertheless, appears to have been upon many other points correct. He believed, Alfonso, Duke of Ferrara, to be an enemy, in some sense a tyrant. Yet the conduct of Alfonso shews nothing but the greatest consideration for the aberrations of the poet and historiographer, and the most kind attention to his comfort. Tasso was evidently a person of constitutional melancholy, probably depending upon the original construction of his corporeal frame. But to abate and check this, no means were adopted; on the contrary, his life, between the ages of eighteen and thirty, appears to have been spent rather in fostering the early seeds of disease than in stifling their growth. The whole time was spent in the pleasures, the dissipations, and the frivolities of courts, excepting what was given to poetry at the universities. No discipline, no self-denial, no self-control, no application of the mind to serious and important pursuits. He wrote, nevertheless, incessantly. But had he endeavoured to confine his mind to the study of history, instead of suffering it to wander in the regions of fancy; had he never relinquished the study of the law, probably the result might have been different.*

3. Among the poets of this country, William Collins and William Cowper will at once occur to the remembrance, as persons with intellectual powers, far above the average rate, yet unhappily subjects of this cruel disease.

William Collins was born at Chichester about the year 1720. He received his classical education at Winchester; then studied at New College Oxford, was admitted a commoner of King's College in the same university, and at length was elected a demy on the opulent foundation of Magdalen College. While at Oxford, he had written many verses, and published what he called Oriental Eclogues. He afterwards attempted odes, and published his performances in 1746; and it is generally allowed by competent judges, that though he has not written much, yet what he wrote bore decided marks of original genius, and poetical ability of a high order. This volume, however, did not at first remunerate his labours. His means were at this time limited; he had no fixed resources; he became dejected, despondent, and with many schemes in his head, spent his days in inactivity, and, it is said, irregularity. The sum of two thousand pounds, left him by a relative in 1750, raised him above anxiety as to pecuniary means. But by this time his mind was so much impaired in energy, that he was in no long time reduced to a degree of childish imbecility. After making an attempt to relieve his disorder by travelling, he was obliged, by the increase of the symptoms, to come home.

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* Milman's Life of Tasso.
Black's Life of Tasso, two volumes.
Memoirs of the Dukes of Urbino. By James Dennistoun of Dennistoun. In three volumes, London, 1851. Volume iii.

A few years more, spent, chiefly at Chichester, in a state of great feebleness of the mental faculties, finished, in 1756, the life of this man, at the comparatively early age of thirty-six.

No exact information has been given upon the particular form of disorder by which the mind of William Collins was assailed. From the few facts recorded, it appears to have been at first a sort of partial insanity, a scheming, fanciful turn of mind, in which he planned great undertakings, but accomplished little or nothing. Samuel Johnson, who knew him, and visited him on his return from France, at Islington, speaks in terms somewhat contradictory. He says in one place that Collins was assailed by disease and insanity; and in another that his disorder was not aberration of mind, but general laxity and feebleness; a deficiency rather of his vital than his intellectual powers. Johnson says also, that when he visited him at Islington, there was nothing of disorder in his mind discernible by any but himself; but he had withdrawn from study, and travelled with no other book than an English Testament, such as children carry to school. It is an interesting feature in his character, that of this book he said, "I have but one book, but that is the best."

When, in his last illness at Chichester, he was visited by Dr Warton and his brother, he spoke not without sense; but a few minutes' conversation exhausted him, and obliged him to rest on the couch.

There is no doubt, nevertheless, that his mind was very much impaired in energy, though it may not be easy to define the exact lesion of the faculties.

Latterly his mind was almost an entire blank. Memory was gone; judgment and reasoning there was none; the imagination, for which he was distinguished, was in a state of total confusion. Yet it is certain that Collins was a person of powerful mind, highly cultivated; and while he possessed the poetical genius and mode of viewing nature, he had also a great command over language in its purest form. His case is remarkable for two circumstances; one, the early age at which the disorder appeared; the other its speedily fatal termination. It may with strong probability be inferred, from what is now seen in analogous cases, that chronic *meningitis* was one of the principal material disorders under which he laboured.

4. Very different, and in some sense opposite, is the case of William Cowper. He appears to have attained his thirty-second year before the malady made a decided appearance; and the circumstance which immediately brought it out appears, so far as can be judged, to have been the mental struggle taking place in the mind of a person of great natural diffidence, when he was contemplating the idea of appearing in an official capacity before the bar of the House of Lords. There is no doubt, at all events, that his apprehensions became so dreadful, that they completely subverted his reason; and after some unavailing attempts by his friends to inspire him with confidence, it was found necessary to remove him, in December 1763, to St Albans, where he resided under the care of Dr Nathaniel Cotton until July 1764, a period of at least seven months. He must at this time have been in his thirty-third year.

The form of the disease which throughout affected William Cowper was melancholy and a despondent state of mind, associated with religious apprehensions; and, at one period, it is stated that he had contemplated suicide.

From 1764 to 1773, a period of nine years, during which he resided in a retired situation in the country, he appears to have remained free from any decided attack, though at all times his mind retained its original depressed and despondent state. In 1773, however, this became aggravated, according to Mr Hayley, by such severe paroxysms of religious despondency, that Cowper required management of the most delicate, vigilant, yet inflexible character. This condition extended through several years, apparently till about 1779. But it is clear from the fact which Cowper himself records, that, in 1774, he began the work of taming the three hares, that his mind must have been beginning to clear up, and the clouds by which it was obscured to be dispersed. It appears, at all events, from various passages in his correspondence, that he was, in the year 1776 and 1777, in his usual state of health; despondent, indeed, occasionally, and apt to take gloomy views of things; but with his intellect clear, and forming, upon various events local and general, correct judgments, and giving just views of various literary works.* In 1778, he read the work of the Abbé Raynal, and gives a most just view of the historical and philosophical merits of that performance.† About the same time, or in 1779, Cowper must have been writing the greater part of those early poetical dissertations which, from *Table-Talk* to *Retirement*, were first published in February 1782. These poems had been written previous to the winter of 1780–81; for this winter he spent in finally revising them and preparing them for the press. *The Progress of Error* was sent to the Rev. Mr Newton in January 1781;‡ *Table-Talk* on the 18th February 1781;§ and about the same time he was occupied on *Expostulation*.‖ *Retirement*, the last of the dissertations, was not commenced until August 1781.¶ In short, during 1780, and the early part of 1781,** he was making writing one of his principal amusements. Yet it is clear that, in July 1780, he was in great apprehensions about his mind, and expresses an aversion to literary labour, under the apprehension that it might aggravate the malady.††

In a letter dated 14th March 1782, Cowper himself states that the composition of those pieces afforded him agreeable amusement at intervals for about a twelvemonth; a statement which shews that the early ones must have been commenced in 1779.

Cowper continued free from serious and prolonged attacks of his malady for about twenty years, between 1774 and 1794, always suffering his habitual melancholy, and occasionally fits of more deep depression, yet capable of great intellectual exertion; in so far as, during the latter eleven years, he wrote the *Task*, many minor poems, and translated the *Iliad* and *Odyssey*. In the depths of this habitual melancholy, he states, he wrote the *Task*, and the volume which preceded it; and in the same depths, he adds, he is now, 3d March 1788, translating Homer.‡‡ It must be understood, nevertheless, that during this long time all were not depths, properly so called. His letters shew that, though never in very elevated spirits, he was capable of enjoying life in a comfortable manner, though at times there were periods of deeper gloom than ordinary. One of these fits was on him in October 1782, at the time at which he wrote *John Gilpin;** and from a note in a copy of Clarke's Homer, it appears that, between the 21st November 1784, when he began the translation of the *Iliad*, and the 25th day of August 1790, when he finished the translation of the *Odyssey*, he was hindered by indisposition for the space of eight months.† This indisposition was mostly of the nature of mental depression. In March 1794, Cowper again fell into a state of extreme mental depression, which continued with scarcely any interval or alleviation until the 25th April 1800, when he ceased to breathe so imperceptibly, that the instant of death was not known. During this period of more than six years, though he occasionally revised and corrected parts of his translation of Homer, and wrote several new poems, he evinced a sort of inert listlessness and mental apathy to every thing,—to his friends,—to the appearances of nature,—to the return of the seasons,—and to all those objects which in days of health had afforded him enjoyment. His mind seemed to have become a sort of blank. Cowper, nevertheless, was free from illusions, if we except that deep solicitude and occasional despair regarding his ultimate salvation, from which he was not at any time perfectly free.

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* Private Correspondence of William Cowper, &c. By John Johnson, LL.D. In two volumes 8vo. London, 1824, vol. i., pp. 31–58.

† Correspondence, vol. i., p. 38.

‡ Ibid., p. 69.

§ Ibid., p. 77.

‖ Ibid., p. 91.

¶ Ibid., p. 134.

** Ibid., pp. 58, 91.

†† Ibid., p. 60.

‡‡ Ibid., vol. ii., p. 119.

In the form of mental disorder, he has been compared to Tasso; but Cowper at no time shewed any of the violence which Tasso at one period evinced; neither was he suspicious, or imagined that he had numerous enemies. His disorder was like that of Tasso and Collins,—no doubt, a form of *Lypemania*, or derangement, with depression of spirits. But it had at an early period taken or received the peculiar religious direction; and this mental peculiarity with the corporeal disease concurred to give his malady those characteristic features by which it was through life distinguished. Other peculiar features in his mental and immaterial constitution, will occur to any one who studies his history with attention. His humanity, his sympathy for others, even for the lower animals, his general philanthropy, all are circumstances which at different periods exercised a decided influence on the character of his disease.

One word may be sufficient upon an idea that has occasionally been expressed, as to the cause of the insanity of William Cowper. It has several times, by different persons, been ascribed to the peculiar character of his religious views. This is to mistake the effect for the cause. The peculiarity of the religious views of Cowper were the effect, and not the cause, of his mental disorder. The mental disorder was connected with the peculiarity of his mental constitution, and his nervous system. There was in the corporeal frame and in the brain of Cowper a peculiar material cause for the mental distress and gloom under which he so long laboured. This prevented him from controlling his reason and imagination in the proper manner. He was, indeed, not responsible for the ideas and illusions under which he laboured, any more than a person that is asleep, or in a fever, is responsible for the visions and dreams which then present themselves to his fancy. It is said that the first cause of the mental disorder was his having checked in very early life an erysipelatous complaint of the face. On the nature of this erysipelatous disorder no information is given. But it was probably one of theeruptions to which young persons are liable, and not true erysipelas. In one of the letters he states, that a melancholy seized him after the death of his mother-in-law (step-mother); and this obliged him, he says, to quit London, and consequently to renounce the bar. It is not a matter of doubt that he was the subject of those illusions which occasionally beset the lypemaniacal.*

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* Hayley's Life. London, 1806, vol. ii., p. 61.

† Ibid., vol. iv., p. 266.

5. Besides the examples now mentioned, not a few others might be adduced, which shew that mental derangement, in certain forms, may co-exist with intellectual powers of a high order. One that will doubtless present itself to the minds of some is that of Dr Swift, who has by many been supposed to have become insane, and to have laboured under *dementia* at the close of life. But this instance is not free from considerable objections. The history of this distinguished man was in 1847 fully examined, and all the evidence for the existence of derangement has been carefully investigated by Mr Wylde, and after very elaborate inquiry, he has seen reason to infer that it is not proved that Swift was either mad in middle life, or mad or imbecile in his latter years, according to the acceptation of these terms by the most able authors.

It is unnecessary to go over the facts of this case after the detailed examination which they have undergone at the hands of Mr Wylde. But it may not be unseasonable to advert shortly to the leading particulars disclosed by the history of Jonathan Swift.

The symptoms by which Swift was afflicted from an early period of life were *vertigo* or giddiness, pain and noise in the head, and deafness. These symptoms were not at first constant; they came in fits, at least the giddiness and pain in the head, after intervals uncertain in length. The giddiness is stated first to have come on after eating a large quantity of golden pippins at one time at Richmond, about the year 1690. The deafness he contracted according to his own account, more than four years after this, that is in 1694, by sitting on a cold bench reading. These symptoms used to attack the patient separately at first; afterwards they came on together, and seldom left him many days or weeks altogether free. We hear little about these complaints, nevertheless, till the years 1710–11, January, when Swift was in London, and when he repeatedly mentions, in the journal to Stella, the giddiness, though with little pain. At this time he appears to have been taking for the alleviation of his complaints bitter medicines and pills, which were most probably chalybeate.

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* Private Correspondence of William Cowper. By John Johnson, LL.D. Two volumes. Preface, p. xv.

And here, before proceeding further, it is requisite to advert to a mistake into which Mr Wylde has fallen,—not very important indeed,—but nevertheless an error. Mr Wylde represents Swift to have been ill with his disorder in 1708, when in Dublin, and in proof of this adduces a passage from a letter from him to Archbishop King. Swift may have been ill at this time, but no direct proof of the fact is recorded; and the earliest mention is made between the years 1710 and 1713, in the journal to Stella, when he was resident in London. The letter in question is one from Archbishop King to Dr Swift, not from Swift to Archbishop King, and it contains an account of an illness which affected the Archbishop in 1696, and for which he was treated by Sir Patrick Dun, at that time one of the principal physicians of Dublin. Swift indeed was not in Dublin during the time at which this letter was written; for his correspondence shews, that he resided in England from February 1707-1708, until the end of April 1709. There seems no ground therefore for the conjectural correction of the year 1696, by suggesting that it should be 1690, as given by Mr Wylde; for the statement refers not to the ill health of Swift but to that of Archbishop King.*

Swift suffered every now and then attacks of giddiness and inability to walk steadily, especially after he had been dining out, and taking more food and wine than was proper. He also continued to take various remedies, which either physicians recommended, or friends similarly afflicted, as Lady Kerry, were taking. These medicines were, as already stated, bitter infusions, and sometimes pills, evidently chalybeates. This state of symptoms appears to have continued more or less regularly the whole time he was in London, that is, till

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* Swift's Works by Scott, volume xv., p. 330. Second Edition. Edinburgh, 1824.

June 1713, when he departed for Ireland as Dean of St Patrick's. Hither the disease did not fail to pursue him; and its symptoms continued to recur with greater or less frequency, and more or less regularly during life; whether he was in England, as was the case in 1714, or in Ireland, in Dublin, or in the country. The complaint increased in severity as years rolled on. Fits of pain, and noise in the head, and giddiness, were more frequent and longer in duration, and deafness was more frequent in its visitations.

These symptoms indicated undoubtedly the presence of chronic congestion of the cerebral membranes; (*meningitis; arachnitis;*) perhaps not general, but nevertheless sufficiently marked and sufficiently severe. Upon these complaints, Swift appears not to have consulted many medical persons. Dr Cockburn appears to have been the only one whose advice he sought or followed; for though intimate with Dr Arbuthnot, and very friendly with Dr Friend, there is no proof of his having had professed medical advice from the former until 1723, and not at any time from the latter. In the month of November 1723, when Swift was in his fifty-sixth year, and his complaints had been more or less present for thirty-three years, Dr Arbuthnot recommended for the *Vertigo,* as he calls the disease, a cure upon which he was to pawn his reputation. The remedy of which Dr Arbuthnot at this time spoke so confidently, was the use of the Geronsterre water, one of the springs at Spa, which was to be drank on the spot. He adds that it had succeeded marvellously with all the patients which he had recommended to use it, except one; and he relapsed only because the course had been too short the first year, and he did not return the second season, Swift had tried the use of Spa water in January 1713, while in London; but as this was the imported water it was not the one, Geronsterre, which was ten years afterwards recommended to his use by Arbuthnot. In 1713, this water was so far from agreeing, that it aggravated the vertigo, and produced œdema of the legs. There is little reason to believe that the Geronsterre could have been more beneficial.

In May 1721 he had a severe attack of ague,* the effects of which in weakening his head continued till July. In September of the same year the giddiness is still tormenting him; and in 1723, it was so troublesome that, as already stated, Arbuthnot recommended for its removal a visit to Spa.† In September 1724, and in April 1725,‡ we find him still bitterly complaining of the recurrence of the giddiness and deafness, in a form so distressing as to incapacitate him from doing many things which he was anxious to do.

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* Correspondence, in Works, vol. xvi., p. 365.

† Correspondence, in Works, vol. xvi., p. 422. Letter from Dr Arbuthnot.

‡ Ibid, vol. xvi., p. 420 and 461. Letter to Lord Carteret.

Swift was now fifty-seven years of age, and had been labouring under the symptoms of cerebro-meningeal disorder for thirty-four years. These symptoms were not indeed constant; but they recurred so frequently, and left him so few and short intervals of health and well-being, that with their long duration, they appeared to be incapable of much alleviation, and to have become in every sense incurable. Indeed, it could scarcely be imagined by any one who took a comprehensive view of the case, that these symptoms could exist so long without being connected with serious disease in the circulation of the brain and its membranes. Swift himself expresses on 12th August 1727, while at Twickenham, the opinion that the giddiness was at length to get the better of him. He was indeed ill,—perhaps not worse than usual,—but still so unwell as to be unable to see strangers. His mind was more depressed than usual, at the prospect of the death of Mrs Johnston. He wanted at this time only three months of sixty years.*

All these symptoms, nevertheless, namely, giddiness, noise and pain within the head, unsteadiness in walking, appear to have been regarded by the medical persons whom he consulted as connected with indigestion, or of a nervous character; in one letter, indeed, Dr Arbuthnot applies to the complaint the term vapours ;† and hence all the remedies which they prescribed appear to have been ordered upon the principle of improving digestion, and fortifying the nervous system. They looked upon *Vertigo* as a disorder of the nervous system, while it was a symptom of serious affection of the cerebral circulation, and the deafness was considered to be the effect of old age. These, however, were the days of symptomatic medicine and symptomatic treatment; and physicians had not hitherto learned the important lesson of looking beyond mere symptoms, and proposing to themselves the question, what is the pathological cause upon which these symptoms depend. Physicians, in accordance with this habit, had plenty of remedies for separate symptoms, but scarcely one remedy for a disease. Hence they had ANTIDINICS or antidotes for vertigo ; and most of the prescriptions which were at different periods given for the complaints of Swift, were of the class called by the old pharmacologists ANTIDINICS.

Of this mode of viewing the disease by which Swift had been so long afflicted, a remarkable instance is presented in a letter from Dr Arbuthnot, dated 13th November 1730. Swift had written to Pope, probably on the 11th August 1729, to the effect that his head was never perfectly free from giddiness, especially toward night, though he had not had a fit of deafness for six months. Upon this, Arbuthnot wrote, stating that he was not alarmed by the passage in the letter to Mr Pope, and that he had found steel, the warm gums, and the bark, all do good in such a disorder. He forthwith gives a whole roll of prescriptions ; *1st*, an emetic of ipecacuanha ; *2d*, an electuary containing steel and wormwood with a little antimony ; *3d*, two dozen pills of assafœtida and castor, great remedies in these days ; *4th*, a vinous tincture of bark with iron ; *5th*, some hartshorn and castor drops. He recommends also, as a serviceable remedy, valerian in doses of one scruple twice in the day, and a bitter and aromatic infusion containing zedoary and wormwood with cardamom seeds, as a means of strengthening the stomach and invigorating digestion.*

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* Correspondence, in Works, vol. xvii., p. 142. Letter to Dr Sheridan, dated 29th August 1727.

† Vol. xvi., p. 422.

All this dosing and medicine-giving, it is to be observed, is directed for an old man of sixty-three years of age, who had been, if not an invalid, at least not free from disorder for forty years, and by a physician of acknowledged talents, great judgment, and good sense. It was probably too late, at that period of life, to expect benefit from any class of remedies ; too late at that stage of a protracted disease to expect that treatment, however appropriate, could be beneficial, and not very rational to suppose that, upon a disorder of the brain of forty years' duration, any favourable impression could be made by any course of remedies ; but probably less evil would have resulted, if the patient had been left to nature, and the disease to itself, than by trying to rouse, by the administration of bitters and tonics, the powers of a stomach weakened by sixty-three years' service, and create an artificial appetite, and consequently make the patient take more food than was good for his condition.

It seems further singular, that, with such symptoms, no restriction was placed upon the amount of food and wine, and no directions as to diet were given. Swift, indeed, appears to have continued not only to take wine to at least the amount of one pint daily, often a good deal more, but to have been occasionally allowed to take brandy. All this was done with the intention of giving strength and energy to the nervous system.

Subsequent to the year 1732, the incidents of the life of Swift were few and melancholy. In December 1732, Gay died; in 1734, Arbuthnot. His intellect remained in considerable vigour. Memory was probably a little impaired. His passions remained as strong as ever. His dislike and distrust of mankind generally; his hatred and hostility to particular individuals, as Sir R. Walpole, Archbishop King, and some others; his vindictive feelings against those who he conceived had injured him, seemed unabated in force. From 1736 and onwards, the fits of periodical giddiness and deafness had returned with violence rather increased. In 1742, he is represented by Dr Delaney to have been in a state of frenzy for several months; and in October of that year, in the site of the left eye a swelling took place, which attained the size of an egg, with redness and discolouration of the eyelid so considerable, that the surgeon apprehended mortification. The acute pain of this swelling kept him awake, it is said, for one month. At length this tumour subsided. For three years after this period, Swift is said to have remained nearly silent, in a state which is ascribed to fatuity, though with short and occasional gleams of sensibility and reason.*

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* Works by Sir Walter Scott, 2d Edition. Edinburgh, 1826. Volume xvii., Pp. 323, 324.

It is unnecessary to follow further the details of this mournful narrative. These have been given fully by Mr Wylde. Life was prolonged in a state of great pain, infirmity, and helplessness, till the 19th October 1745, when Jonathan Swift expired in his seventy-eighth year.

The head was opened by his relative Mr Whiteway; but all that we learn is, that water, that is, serous fluid, was found in the sinus, probably understanding by this term the cavity of the ventricles.

In the year 1835, it became necessary to repair the foundations of St Patrick's Cathedral, in consequence of injury done to the building by inundations of the Poddle river. The remains of Dean Swift, which had been deposited there ninety years previously, were then raised, along with those of other persons; and a minute examination of the skeleton, and the skull particularly, was made by Dr Houston, chiefly with the view of knowing, whether any additional light could be obtained on the nature of the disease by which Swift had been so long afflicted. Of the state of the brain or membranes, of course nothing could be learned. But it is well known to those conversant in anatomical inquiries, that, often the bones, and especially those of the cranium, retain for a long time the traces which have been impressed upon them by the morbid action of the covering and the contained soft organs. In the instance of the skull of Jonathan Swift, various marks of this kind were found still to be perceptible after the interval of ninety years.

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* Scott's Life, in vol. i. of Collected Works, 2d Edition. Edinburgh, 1826. Pp. 451, 452.

The following statement is given by the late Dr Houston. "It is my opinion, that the bones cannot be regarded as free from indications of previous chronic disease. There are certainly no marks of caries or of fungous growths on any part of the head; but the condition of the cerebral surface of the whole of the frontal region is evidently of a character indicating the presence, during lifetime, of diseased action in the subjacent membranes of the brain. The skull in this region is thickened, flattened, and unusually smooth and hard in some places, whilst it is thinned and roughened in others. The marks of the vessels on the bone exhibit, moreover, a very unusual appearance; they look more like the imprints of vessels which had been generated *de novo,* in connection with some diseased action, than as the original arborescent trunks. The impressions of the middle arteries of the dura mater are unnaturally large and deep, and the branches of those vessels which pass in the direction forwards are thick and short, and terminate abruptly by dividing into an unusual number of minute twigs; whilst those of the same trunks which take their course backwards are long and regular, and of graduated size from the beginning to the end of their course."

The following description of the inner surface of the skull also deserves attention.

"At the base, the surface was roughened in the sphenoidal region; the processes prominent and sharp-pointed; the foramen magnum of the occipital bone irregular, and the condyloid processes projecting into it. Some parts in the occipital fossæ, the supra-orbital plates, and other portions of the skull, were so thin as to be transparent. The marks of the arteries of the dura mater on the vault were large and deep, but the general surface of the interior of the vault was smooth. Along the line of attachment of the falx the bone was porous, from the multitude of small foramina which had transmitted blood-vessels from the dura mater to the bone in that situation. Above the frontal protuberances (in the region of Benevolence) the bone was thickened, apparently by a deposition of bony matter on its inner surface,—making the inner surface at that part, on both sides, flat in place of concave, and smoother than the other parts, which was the more remarkable, as the other portions of the skull were rather thin. Below, or anterior to that flattened space, about a dozen of small, deep-fissured foramina existed, in a cluster of six or seven on each side, apparently indicating a fungous state of the dura mater at that place. Some foramina in the middle basilar fossæ of the skull were observed, similar to those just noticed, and evidently arising from the same cause. The exterior surface of the skull was smooth and natural. The skull shewed clearly increased vascularity of the dura mater in the basilar and anterior regions. The anterior fossæ were small, both in the longitudinal and in the transverse directions. The middle fossæ were of ordinary size; the posterior fossæ very large, wide, and deep. The internal parts corresponding to the frontal protuberances were unequal in concavity: at neither was there any depression corresponding to the great prominences on the outer surface. The two hemispheres were regular and symmetrical."

In another account by Mr Hamilton, the following remarks are made.

"The side view shewed great elevation above the level of the horizontal line drawn through the meatus auditorius externus. The front view exhibited extreme width of the forehead, large frontal sinuses, and very well-marked external canthi. The orbits were very large, and the orbital plates of the frontal bone very flat, allowing room for great development of the anterior lobes of the cerebrum, and great width of the root of the nose, making the space between the orbits unusually large. On the inside of the upper segment of the skull the groove for the middle meningeal artery was remarkably deep, as were also the depressions for the glandulæ Pacchioni. The frontal bone was very thick, but the osseous structure did not appear to me to be diseased. It was, however, when looking into the interior, and examining the base, that the wonderful capacity of the skull became apparent. From the flatness of the orbital plates, and the great width of the forehead, the room for the anterior lobes of the cerebrum was very great; the depressions, also, for the middle lobes, were very deep. Although, viewed *externally*, the cerebellum would have been pronounced large, yet, in consequence of the tentorium having been exceedingly low, the cerebellum must have been very small, and the posterior lobes of the cerebrum, consequently, very large. In the temporal regions the skull was thin and semi-transparent; the frontal sinuses were small, though their external appearances would have led to a different conclusion."

Though the facts observed by both anatomists are the same, yet there is some degree of contradiction in the inferences deduced. Dr Houston thinks that the inner table of the skull indicates the presence during life of morbid action in the membranes of the brain. To Mr Hamilton the osseous structure did not appear to be diseased. To us it appears, that the roughened aspect of the internal table in the sphenoidal region, the sharpness of the processes, the numerous small foramina along the line of the falx, and on other parts of the internal table, must all be regarded as effects of an abnormal and unhealthy state of the circulation within the cranium. On this subject the following remarks by Mr Wylde, do not exactly accord with what has fallen under our own observation.

" Most of the so-called pathological appearances here detailed (namely, those by Dr Houston), are, however, it is well known, common and natural occurrences in old crania, and nowise indicative of disease. The foramina alluded to are no evidence whatever of a fungous state of the dura matter; but the deep sulci for the meningeal arteries are certainly abnormal, and shew a long-continued excess of vascular action, such as would attend cerebral congestion."

Probably some of the pathological appearances mentioned by Dr Houston, are seen in the *crania* of the aged, not as effects of actual disease; and to this head may be often referred the deep furrows made by the meningeal arteries. But the roughened state of the inner table, especially when attended with a sharp ragged condition of the processes, we have seen so often connected with disease of the base of the brain and its coverings, that without strong evidence that the individual during life had no symptoms of head disorder, we are unable to regard it in any other light than as an effect of irregular and perverted circulation.

The main questions, however, are, Was there disease or disorder of the brain during life? Was the mind of Swift deranged at any period of his life?

It seems scarcely possible to doubt, that there was disease of the brain, and of its circulation, or of the circulation of the membranes during life. The giddiness so often occurring, and always in a more severe form, the unsteadiness in walking, the noise within the head, all point to the existence of a disease of the vascular system of the brain; and while the long duration of this disorder, and its resistance to such means as were employed, are considered, it would be altogether idle and captious to doubt the existence of such a disorder. Even independent of the evidence of inspection after death, this must be admitted to be the fact. Inspection could shew in such circumstances only the effects of disordered circulation of long continuance. We know, however, from parallel and analogous cases, the appearances that may be expected; and we are entitled to reason from the symptoms during life, as well as the scanty information afforded by inspection after death, that the circulation of the brain and its membranes was in a morbid condition.

It cannot be justly said, nevertheless, that this disease had materially shortened the life of Swift. Probably had it not been present, four or five years might have been added; but they with equal probability might have been years of labour and pain, sorrow and suffering, if the subject still retained sensibility. Of a person who dies at the age of seventy-eight years, it can scarcely be said that the life is prematurely ended, or materially abridged. All that can be said is, that had the disease of the brain never been present, life might have been for the last thirty years spent more comfortably and more agreeably. But even to this, there were, as shall be presently seen, obstacles and impediments in the moral constitution of Jonathan Swift.

Regarding the question of derangement, different opinions have been entertained. Most of the biographers of Swift have adopted the view, that in the latter period of his life, the mental faculties were deranged. Johnson states, that "the disorder, which he contracted early," that is, about 1690, "was giddiness, with deafness, which attacked him from time to time, pursued him through life, and at last sent him to the grave deprived of reason." Johnson afterwards adds, that about 1737, his "ideas wore gradually away, till at last his anger under frequent vexations was heightened to madness; that he grew more violent, and his mental powers declined till 1741, when it was necessary that legal guardians of his person and fortune should be appointed;" and represents his madness to be compounded of rage and fatuity. Some of these expressions are vague; others are mere figures of speech.

Sir Walter Scott takes a similar view. He represents him after 1736, as suffering so much from fits of giddiness and deafness, that he could not enjoy conversation or occupy his time by writing; and states, that after July 1740, his understanding totally failed, and his state was first that of violent and furious lunacy, or outrageous frenzy, and then a sort of stupid fatuity.*

It would be tedious and unprofitable to consider all the testimonies given as to this concluding scene of the life of Swift. It used to be formerly mentioned, by Dr James Gregory, in his lectures as an instance of great genius passing in the decline of life to the state of idiocy.†

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* Life, in Volume First of Collected Works, p. 449, 450, &c.

† Conspectus, caput IX., cclxxi. "Aliorum morborum plerumque comes et signum, nonnunquam tamen princeps vel solus Vertigo morbus, per intervalla rediens, paulatim ingravescens, functiones animi pariter atque corporis impedit et labefacit: neque firmissimum et vividissimum SWIFTII ingenium tanto malo potuit resistere."—Editio 5ta, 1815, p. 88.

Very much in the same terms, Dr Gregory used to express himself in his

The correctness of the whole of this representation, is very much called in question by Mr Wylde, who, without entering into the question of what constitutes lunacy or idiocy, thinks that he has sufficient grounds for the assertion, that up to the year 1742, Swift shewed no symptoms whatever of mental disease, beyond the ordinary decay of nature. He allows, that it is certain, that towards the end of 1742, the cerebral disease under which he had been so long suffering, by producing effusion, &c., destroyed his memory, produced paralysis of the face and the right side, and rendered him at all times ungovernable in his anger; but all this, he adds, was the result of physical disease, in one whose constitution was of great nervous irritability, and who had long survived more than the years of a man. The circumstance of Swift not speaking, and observing silence during the latter two years of life, Mr Wylde regards as the result neither of insanity nor imbecility, but ascribes partly to paralysis of the muscles by which the mechanism of speech is produced, partly to loss of memory of the matters which he wished to express. Swift was often desirous to attempt to speak his mind, but could not recollect words to express his meaning. He then was wont to shrug his shoulders, shake his head, and sigh heavily. The circumstance of the law having appointed guardians of his person and property, Mr Wylde further argues, is no proof whatever of his insanity; for there are numerous instances of persons in which the law very properly interferes with a man's estate, although he may not be either legally or physiologically insane, but simply incapable of managing his affairs; and it must be borne in mind that Swift had no family, or any near relatives, to look after him in his latter years.

"We only wonder," concludes Mr Wylde, "that Swift did not become deranged years previously: with a mind naturally irritable, a political intriguer, peevish and excitable; his ambition disappointed, his friendships rudely severed, his long-cherished hopes blighted; outliving all his friends, alone in the world, and witnessing the ingratitude of his country; while, at the same time, he laboured under a most fearful physical disease, in the very seat of reason, the effects of

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lectures, when, treating of the subject of Vertigo, which, as in his Conspectus, he considered both as a symptom of Apoplexy, Epilepsy, Hysteria, Syncope, as well as various morbid states, as drunkenness, sea-sickness, &c., and as a substantive or independent affection.

A similar view of the nature of *Vertigo*, as a symptom of other diseases, and also as a primary affection, is given by Dr Willis in 1672, fully one hundred years before, in the second division of his Treatise De Anima Brutorum: Pars Pathologica. Lond. 1672.

which were of the most stunning character, and serving in part to explain that moodiness and moroseness of disposition, which bodily infirmity will undoubtedly produce; we repeat, we only wonder that his mind did not long before give way. But that Swift was either mad in middle life, or mad or imbecile in his latter years, as tried and tested by the meaning and definition of these terms, as laid down by the most esteemed authors, we again assert, has not been proved."

Such are the conclusions at which Mr Wylde arrives. Some of these inferences, it may be observed, are explanations of certain phenomena; others are nearly equivalent to admitting that the mind of Swift was impaired, perverted, disordered, in his latter years.

A great deal of the determination of the question depends upon the view taken of what constitutes derangement, and what was the particular form of derangement. It may be admitted that the intellect of Swift was sound till probably within a few days or hours of his death; that his judgment was correct so far as it went. His genius certainly underwent that form of enfeeblement which happens to all in the course of old age, and which takes place whenever the circulation of the brain is seriously and permanently disordered. He was, in short, free during the greater part of his life from intellectual insanity, or derangement of the intellectual faculties, properly so called. The same, however, can scarcely be said as to his freedom from a certain degree and form of moral insanity, or disorder of the moral perceptions and affections. By making this statement we do not impugn the humanity, the sympathy, the patriotism of Swift, or any of those qualities which have been by all almost conceded to him. But when the violence of his fits of anger, the furiousness of his hostility against many persons, the outrageous manner in which he sometimes evinced his dislike and hatred of particular individuals, even his behaviour to the two ladies whom he invited from England to Ireland, are remembered, it is not easy to account for them upon any other principle, than that of a certain degree of moral insanity. His conduct to Miss Van Homrigh upon the last occasion on which he visited her, is more like the savage and violent treatment of a madman, than the behaviour of any person either of sense or feeling. Nor can the obstinate and sullen manner in which he refused to Mrs Johnson, on her death-bed, the acknowledgment which she requested, be considered as anything but the act of a very selfish and very unreasonable person, in whom the sentiments of justice are entirely extinguished.

Even his writings, abounding as they do with marks of great ability, both in thinking and in expressing thoughts, afford multiplied proofs of a certain degree of tortuosity and perverseness in the moral sentiments. These writings, both in verse and prose, abound in filthy and offensive images to such a degree, that it would be quite impossible to read them aloud, or even to recommend their indiscriminate perusal to the young. If it be said, that this was in some sense the fault of the age, the answer is, that though there were much coarseness of language, and often obscene and filthy illusions occurring in the writings of others, yet there is in no other author the same frequent and almost habitual exhibition of thoughts and figures of the most indelicate and disagreeable character. Mandeville, in his Fable of the Bees, goes far enough in this way as to coarse language and rather indelicate sentiments. But he is greatly behind Swift in the frequency and abundance of the allusions and figures of this character. The coarseness of such pieces as the Problem, the Lady's Dressing-Room, the Verses for Fruit Women, &c., and many others, is so gratuitous, that it seems wonderful, that any one could deliberately devote time in making such records of such thoughts. If the ideas arose, what was the use of taste, of morals, of reason, but to suppress them. This however Swift evidently never attempted. He rather cherished thoughts and images of this kind, and seems to take pleasure in dwelling upon them.

That all this sort of thinking and writing was not solely the fault of the age, the example of Addison most clearly and indisputably shews. In none of the papers of Addison, and none of his poetical productions, and not even in the writings of Steele, do we find any of those indelicate and offensive images which constitute so large a proportion of the writings of Swift.

To explain the love of Swift for this style of writing; to account for the mixture of deliberation and hostility which is found in many of his writings, and the general vehemence of his passions, seems hardly possible, without supposing that there was a certain amount of derangement in the moral sentiments. To say, as Mr Wylde has done, that Swift had a mind naturally irritable; that he was a political intriguer, peevish and excitable; that his ambition was disappointed, his friendships severed, his cherished hopes blighted, may explain the facts in one way; but this explanation does not remove these facts from the head of a degree or form of moral insanity. These circumstances we suggest to the consideration of those whose minds are not decidedly fixed on this subject, with the additional remark, that if several similar cases be compared, there may be ground for referring to the head of moral insanity,—that is, derangement and disorder in the moral sentiments,—the peculiar eccentricities and oddities which marked throughout life the career of Jonathan Swift.

It is a law of the moral constitution of our nature, that the indulgence and gratification of the malevolent and resentful passions, entails upon him who obeys their impulses, as much, if not more, suffering and misery, than upon the person upon whom their vengeance is directed. The blow which the angry and vindictive man directs upon the object of his rage, recoils upon himself with equal force, and not unfrequently with more hurtful effects. For injuries, real or supposed, we punish, or try to punish, those who we believe have done the injury. But we seldom think that, in so doing, we are rearing within our breasts a serpent brood, which will at length prey upon ourselves, and devour the frame in which they are reared. The most complete vengeance ever inflicted by a mortal arm never does so much injury to the object of its wrath as to its author. This was a lesson, however, which the Dean, with all his knowledge of mankind, had not learned, or had not understood in due season. With many who offended him; with many who he believed had injured him; with all whom he disliked, he lived in more or less animosity, sometimes in open hostility and warfare. Such a life of restless annoyance was not calculated to insure mental tranquillity. It doubtless aggravated the disease under which he was labouring. It could not probably be said to be the cause of the disease; perhaps it was an effect; but it was itself a proof and effect of a mind not perfectly sound.

These views would lead to the inference, that Swift was not, strictly speaking, responsible for some of his violent and outrageous proceedings. This part of the question, however, cannot be investigated at present.

6. Somewhat different, but less equivocal, is the case of Blaise Pascal, which indeed resembles in some respects that of the poet Cowper.

Blaise Pascal was born at Clermont, the capital city of the province of Auvergne, on the 19th of June 1623. In 1631, his father, Stephen Pascal, went to Paris, carrying thither his family, and there fixed his abode. The father took the charge of educating his son, and taught him both the ancient languages and the elements of mathematical science. In the latter he made progress so rapid, that at the age of sixteen he composed a Treatise on Conic Sections, which roused the doubts and excited the admiration of Descartes.

Intensity of application, conjoined with a feeble and delicate frame, rendered his health at eighteen years so uncertain, that after that age he seldom passed a day of his short life without pain. On the 19th of September 1648, he performed his noted experiment, of ascertaining the weight of the atmosphere, by ascending with a barometer the Puy de Dome. He was then twenty-five years of age. This was the last occasion on which he applied his mind to human science and physical inquiries. After this he devoted himself entirely to religion, morals, to speculations on these subjects, and to the practical duties of religion. It was his principle, that Christianity obliges us to live entirely to God, and that we should have no other object but the Supreme Being. He accordingly renounced all secular pursuits; and, retiring from the world, became a member of the Society of Port-Royal. Concerning the peculiar form of bad health under which he laboured, little correct information has been preserved. His sister merely informs us that he was distressed by continual maladies, which were constantly increasing in severity; that he had intolerable pain of the head, and excessive heat in the bowels, and many other evils. For the alleviation or removal of several of these symptoms, the physicians directed that Pascal should take purgative medicines once every two days for the space of three months. To any one this discipline probably would have been irksome, if not severe. To Pascal it was absolute torture; in consequence of his being unable to take medicines except warm, and then only, from some organic impediment or idiosyncrasy, drop by drop,

The administration of these remedies, with others employed, was followed by some alleviation of the symptoms, but not by perfect health. The physicians accordingly arrived at the conclusion, that in order fully to re-establish the health of Pascal, it was indispensable that he should abandon all mental application of every kind, and that he should, as far as possible, seek for opportunities of amusing his mind. This advice, his sister Madame Perier says, Pascal felt great difficulty in following, because he saw the danger arising from it; but at length he followed it in some degree, feeling himself obliged to do all in his power for the re-establishment of his health. Pascal believed, that to pay much attention to health was a sort of sensual indulgence, in short a sin.

Another sister had already become a Nun, and, renouncing the world, had retired to Port Royal, where she led a life so sanctified, it is said, that she edified the whole establishment. By her persuasion Pascal entirely quitted the world, and resolved to retrench all superfluities and the use of all comforts, at the risk of endangering his health, because he believed salvation to be preferable to all things.

He was then in his thirtieth year. From this time to that of his death, in his thirty-ninth year, he continued strictly the same mode of life.

It is scarcely requisite to enumerate the austerities and petty offices in which this eminent man spent his life. They are all of the kind believed by strict Roman Catholics to be essential to a Christian life. He made his own bed; he carried his meals from the kitchen himself to his apartment; avoided all use of servants; the rest of his time he spent in reading the Scriptures, and studying commentaries upon them, in performing religious duties, and in attending the services of the Church.

It is not difficult to perceive that in this short narrative is contained the history of an overworked brain, ENKEPHALOPATHIA MENTALIS; the description of one whose whole time has been spent in cultivating the mental faculties, to the exclusion of the bodily organs; and the formation in early life of that most dreadful and incurable of all distempers, a state of general nervousness; nervousness of body and nervousness of mind. The end was not remote.

In the year 1654, Pascal was going for a drive along the side of the bridge of Neuilly in a carriage and four horses, according to the custom of the day. When he was near the bridge the two leaders, from some fright became unmanageable, seized the bits in their teeth, and plunged into the river. Fortunately the traces gave way, and the carriage was left on the bank. The sudden and violent shock which Pascal received nearly cost him his life, and so disturbed his imagination, that from that time forward he believed he saw a deep precipice always open at his side, and ready to swallow him up. This was an illusion; but it was the creation of the fancy of one in whom the nervous system had been previously weakened and disordered by early over-exertion of the mind, long seclusion to an inactive life, and the gloomy and desponding thoughts which previous disorder of the brain had engendered.

"The actual precipice in which his reason was engulphed," says M. Aimé Martin, "was the doubt upon all metaphysical subjects which occupies superior souls,—a terrible doubt from which the positive practices of Christianity alone can deliver the mind. When we read that Pascal was in the habit of carrying under his garments a symbol formed of mystical words, we feel that his powerful mind had retreated to these superstitious practices in order to escape from a frightful uncertainty. This was his terror. The imaginary precipice which, after the occurrence of an accident nearly fatal, the weakened senses of Pascal believed to be yawning under his feet, was only a faint image of that gulf of doubt and apprehension which formed the internal terror of his soul."

From some inaccuracy, there is a little discordance in the date of this event. It is said to have taken place as already stated, in the year 1654, and if it did so, this must have been when he was in his thirty-first year. At all events, after this accident, his complaints were evidently aggravated; and for the last four years of his life, it was truly a long disease, both of body and mind. About the year 1658, he had toothache so severe, as to be deprived of sleep for many nights. The pain of the head also never left him. In June 1662, he took a great loathing to food; for which his physician recommended him to abstain from solid food and to employ purgatives. On the 29th of June, he had come to the residence of his sister Madame Perier, and three days afterwards, he was attacked with violent colic. These pains, however, he bore patiently; he rose daily, and took remedies. It is stated that this was not attended with fever, and that the pulse was good. On the fourth day of the disorder he confessed; but as he observed that this alarmed his friends he deferred, in the mean time, taking the sacrament.

Subsequently, he was desirous to take the communion; but this the physicians opposed, saying that he could only take it fasting, unless it was in the night. This he did not find convenient to do without necessity; and also, to take the communion *in viaticum*, it is requisite to be in danger of death, which the physicians did not think he was.

As the colic, meanwhile, continued, the physicians ordered him to drink the mineral waters, which afforded great relief. On the 6th of August, he felt great giddiness, with great pain in the head. This symptom, the physicians ascribed to the use of the waters, and expressed no alarm. Pascal, however, though the colic pain was relieved, believed himself approaching his end, and requested that the sacrament should be administered to him, answering to their remarks, that his complaint was not understood, that they were mistaken, and that his pain of the head had something extraordinary.

The pain in the head went on increasing. About midnight, on the 17th of August, he had a convulsion fit so violent, that when it was over, his relations thought him dead, and they suffered the agony, as they thought, of seeing him die, without having received the holy sacrament, after having requested it so often, with so great importunity. Pascal, however, was not gone. He recovered his consciousness and judgment, it is said, as in his best health. The clergyman then was in attendance; Pascal received the holy viaticum and extreme unction, with so much emotion, that he shed tears abundantly. Soon afterwards the convulsions returned, and left him not one moment of integrity of mind. They lasted till death, which took place twenty-four hours afterwards, on the 19th day of August 1662, at one o'clock in the morning, when he was aged thirty-nine years and two months.

Such was the early termination of the short and melancholy life of Blaise Pascal. Apart from the constitutional *Lypemania* under which he suffered, his mind was tinctured with those stern and dark principles of Asceticism, which made him look on the earth as a place, not for enjoyment, but for trial, suffering, and self-denial, of every kind. Disease he regarded as a positive benefit; health as a dangerous and seductive condition; poverty, as an advantage; wealth, as, if not an evil to the possessor, yet only to be used for providing for the poor, and relieving the distresses of our fellow-creatures; self-denial of every kind, in food, clothing, accommodation, are the duties of all Christians. We ought to live as if at all times ready to die. These sentiments, it is impossible to deny to be just. They may be carried so far, however, as not only to disable us from performing our duties in the present life, but to prevent us from taking any pleasure in the beauties of the natural world, and may lead us to regard as criminal, the delight with which we contemplate the sea, a fine river, mountain scenery, and the diversified surface of a fertile and well-cultivated country. That the mind of Pascal was at once strong and weak, may be inferred from the fact, that during the latter years of his life, he was in the habit of going on particular days to those churches in which relics were exhibited; and for this purpose he kept a kalendar, in which were noted all the days on which such exhibitions took place. Notwithstanding his constant study of the Scriptures, he seems to have thought that the Christian religion consists in being present at solemn masses and church services.

The derangement of Pascal was totally different from that of Swift, of Collins, of Cowper. It was manifestly induced by the overworking of the brain in early life, by constant intellectual labour, by neglecting to strengthen the corporeal frame, and thus inducing a delicate nervous state of the system. The constant headach which he suffered undoubtedly indicates the presence of disorder of the cerebral circulation; and from the mode in which life terminated, it is scarcely possible to doubt that the brain was the seat of disease. It may not, however, have been the sole seat of disease. The alimentary canal was undoubtedly affected at first in function, afterwards probably in structure. The multiplied and constant sufferings of his body appear to have acted on his nervous system and mind, and to have impaired the energy of a mind naturally powerful. Where was the infirm point becomes the question. It was neither in the judgment nor in the intellect. A dark, gloomy cloud appears to have involved his mind, especially after the accident of 1654 at Neuilly, and made him view every thing through its medium. We are informed by some of the commentators on his life and writings, that after this event his reason was entirely overthrown and subdued by the fatalist dogma of Jansenian Grace. He imagined that he had previously led a life of great profanity, because he had lived as others did, and did not practise the austerities in which he subsequently lived. Religious belief, nevertheless, and religious anxiety, were not the causes of his illusions. He took those views of religion and of Christian duties, because his imagination was disordered, and reason, though still present, did not practically exercise the controlling power.

The aberration of Pascal is in many respects not easily understood. Between the beginning of January one thousand six hundred and fifty-six, and the beginning of June one thousand six hundred and fifty-seven, in the space of eighteen months, he wrote the nineteen *Provincial Letters;* a work unrivalled in French literature for conclusive reasoning, accurate thinking, and correct language. In style it has been allowed to mark an era in the history of the French language. He attacks and subverts the doctrines and practices of the Jesuits, with facts, unanswerable arguments, sarcasm, irony, and sometimes even pleasantry. It is a grave and serious work, enlivened with frequent flashes of wit and humour, written in the most correct and polished language. Yet during this time the mind of the author was darkened by the deep cloud which overcast his imagination. He examines, in no very reverent manner, the false devotion paid to the Virgin Mary introduced by the Jesuits ; ridicules the idea of saluting the Virgin when her image is met in the streets ; the practice of counting beads ; and saying daily *Ave Maria* in honour of the heart of Mary ; yet the same man went to every church in Paris to behold the bone of a dead man, and, if we can believe history, put implicit faith in the story of the Holy Thorn, which was represented to have cured his niece of *Fistula lacrymalis.* Theoretically he was a rational, judicious, and sincere Christian ; practically he was not free from superstition and credulity.

7. Different from any one of the instances already considered, yet presenting phenomena which distinctly enough refer themselves to disorder of the mental faculties, was the case of Rousseau. In the mind of this remarkable man, vanity and conceit were strongly predominant ; but not less marked

were the restlessness, the foolish and unfounded suspicions, the apprehensions, and the discontent, with which his whole life was filled. Banished from France by a decree of the parliament, in consequence of the doctrines taught in *Emilius*, he believed that all the governments of Europe conspired in persecuting him, that all the learned societies and learned men of Europe were in a conspiracy against him and his opinions; that the clergy and his neighbours wherever he went were his enemies; that plots were incessantly laid against his safety, his happiness, and his reputation; that he was calumniated and misrepresented; in short, that he was the most miserable and oppressed man in the world. He represented himself to be in great poverty, when it appears that he was not. With those who befriended him, or offered to befriend him, he lived in a state of constant suspicion and ill-humour. Professing admiration and esteem, he treated his friends with caprice and ingratitude. His mind was effeminate, and his sentiments affected. Inquietude of mind, and restlessness of body; a most exalted idea of his own importance, suspicious of most other persons, the habit of putting the worst constructions upon the best actions, of ascribing malevolent motives to others, causeless and ungrounded offence; these were the usual, the habitual proceedings of Rousseau. It is unnecessary to dwell upon the case. But good observers during his life,—all those who knew the man, Hume, Burke, D'Alembert, Turgot, and many others,—arrived at the conclusion that his behaviour was that of an insane person. It was monomania, with immense vanity and self-esteem, not a little insincerity and hypocrisy, and much disregard to truth.

This man, nevertheless, was a person of undoubted talents, great imagination and invention, infinite power of happy conception, and beautiful writing. When he wrote, his views were philosophical, extensive, and judicious. In common life, and in intercourse with others, his acts were those of a madman, not unmixed with knavery. Many of his suggestions on education are admirable, and most excellent; his example would have rendered any pupil contemptible, and the object of pity, not improbably of abhorrence, if he shewed the same want of candour, or practised the same disregard for truth, and lived in the same habitual violation of the laws of morality.

It is unnecessary to advert to other instances of persons of powerful intellect, in whom the mind became deranged. The instances are manifold, in all sorts of men, in all professions. We have hitherto looked merely to instances of literary persons; but many in other departments might be mentioned.

In the instances already mentioned, it is manifest that the derangement is very much confined to one set of faculties. The intellect, the genius, seems to be little, perhaps not in any degree, impaired. At all events, it possesses sufficient energy to be capable of great intellectual efforts. There is one species of derangement, in which, without perceptible affection of the intellectual powers,—that is the power of studying scientific subjects,—there is, nevertheless, an erroneous and deranged mode of thinking. The individual does not study, or with perseverance follow his own pursuits. He is restless and discontented, and thinks himself capable of excelling in all other departments, as a politician, a statesman, as an orator, as a judge, and guide to others. He discovers defects, errors, and evils, in all human and social institutions. These he resolves to reform ; this task he undertakes. These subjects occupy his whole thoughts, to the exclusion of his own pursuits, which are more or less neglected. He lives in a constant hurry and agitation. He has plans for improving the condition of the poor and working classes ; for improving and rectifying the municipal institutions of the city in which he lives ; for reforming all the errors, and amending all the abuses of government. Some of the plans may be good ; many are chimerical ; all are impracticable. He finds indifference, opposition ; he proceeds from one to the other scheme ; from one to the other subject. He attends to everything but his own affairs. This may go on for a considerable time, no one daring to say that the man is deranged ; or if any say so, he is disregarded. This, nevertheless, is a form of derangement not unfrequent ; and many physicians can easily recall the originals from which the sketch now given is taken. At length, after weeks or months of this feverish irritation, he may give proofs in his conduct of unequivocal derangement ; or he may merely proceed in his restless career, acquiring a degree of notoriety which satisfies the great body of common observers that there is decided disorder of the faculties. This disorder, nevertheless, it is by no means easy to define ; and it is extremely difficult, on the application of the ordinary tests, to prove the man to be deranged.

Another peculiarity of this class of persons is a heedless and irregular mode of spending money. They enter into wild, extensive, and chimerical undertakings, requiring considerable pecuniary supplies. If they have means or property of their own, these are squandered in the most thoughtless and unprofitable manner ; and in no long time their whole resources are dissipated and exhausted. In some instances, large purchases of articles which can be of no manner of use, at least in their hands, are made; in others, money is invested in engagements in which no sane person would embark one shilling.

To persons of this turn of mind, political movements and periods of public and social agitation are most dangerous. Their minds being, as it were, in a constant state of combustion, and their emotions ready to be excited by the most trivial circumstances, they are immediately inflamed with the increasing temperature of public agitations. Prone to form the most extravagant hopes from the effects of their exertions, they in general are active in making converts to their opinions. But as these opinions are seldom practicable, and almost invariably wild and unreasonable, the opposition which they meet, and the neglect and indifference with which they are treated, very often increase their ardour until they became violent and unmanageable. In this state they commit overt acts, which require to be repressed by authoritative means, and which indicate either ungovernable temper or actual derangement, or both.

It is supposed to be either unjust or uncivil to pronounce persons of the kind now described to be deranged. Strictly speaking, perhaps, the peculiar disorder of their thinking faculties does not come under the ordinary definition of mental derangement. Many of those persons possess, not only good natural talents, great acuteness, and much ingenuity, but they have correct, and often extensive, information. But there is, in their whole mode of thinking and acting, a hurry, confusion, and eagerness, which prevents them from applying their faculties steadily to any proper object. The subjects they select injudiciously. They would be politicians, financiers, reformers. Their own affairs they neglect. There is imprudence and want of sense in their whole conduct. There is little doubt that the disorder by which these persons are afflicted, is a peculiar species of mental derangement. They are guided in their actions by emotion and passion. Reason and reflection have no influence, no control, over their thoughts or their conduct. Of some, the conduct is not unaptly designated by the terms flighty and flightiness. Others, however, evince constant discontent with their present position, however good, restlessness in mind and body, and the most confident belief that, were they in a proper position, they could new-model all society, and reform all existing institutions. They are said to be excitable, and to be in states of excitement; but they will not allow that they are deranged.

Without, however, dwelling longer upon the general characters of this form of insanity, we shall give a case which occurred not long ago in Paris, and which well illustrates some of the peculiar features of the disorder.

8. M. Auguste Isaac B., professor of mathematics at Paris, was an old pupil of the Polytechnic School, where he had distinguished himself by his speedy mastery of geometry and its practical applications. At one period of his life, which refers to the year 1828, M. B., whose mind was then inclined towards mystical ideas, made extensive researches upon the truth of different forms of religion, and particularly that of the Catholic religion. He had been born a Jew, and was educated in the Jewish religion; but he was at this time ready to abandon it and embrace the Catholic form of Christianity, if the latter religion appeared to him the only true one.

In 1831 he was admitted to the Polytechnic School, but was subjected in 1832 to a temporary rustication, in consequence of having forced the deposit at the time of the interment of General Lamarque.

This dismission, however, would appear rather to testify in favour of the loyalty of his character; for it was to the interrogation of one of the heads of the establishment, who had no certain information that M. B. was among the number of the transgressors, that he candidly declared that he had disobeyed the orders of the general.

After leaving the Polytechnic School, he devoted himself with success to the business of teaching mathematics; and for a long time he earned in this manner an honourable subsistence. In the year 1837, according to his own account, a singular episode in his life took place. Convinced that the study of the sciences is so disagreeable and laborious to young persons solely because it is made purely theoretical, he took the resolution of uniting practice with theory. He became, accordingly, a mason, and abruptly abandoned his occupation, in order to work with the chisel and the trowel in his hand. He became successively conductor and inspector of the Versailles Railway on the left bank of the Seine.

In 1838 M. B. entered into the matrimonial state. At first he was very happy; but afterwards he complained of numerous crosses which he ascribed to the character of his wife, and the life which she led. For these crosses, on the other hand, the wife assigned a cause totally different. According to the account of this lady, her husband, who by his lessons had acquired a respectable competence, conceived then the project of paintings for illustrating the polytechnic sciences upon the model of the paintings of Las Cases. This undertaking, in the performance of which he published twenty paintings, would, according to his own statement, require five hundred. It caused a great expenditure of money, made him contract very heavy pecuniary engagements, and did not succeed. When he visited the asylum of M. Brierre de Boismont, on account of a relation, he was already in embarrassed circumstances, and was under legal demands; but instead of thinking of these evils, he said that the affair was excellent, and that it contained the foundation of a large fortune. To the very sensible observations of his wife, he answered, taking aside the director of the establishment, that she was liable to fits of absence, and that he was afraid that he should be obliged to bring her to the establishment.

The mental activity of M. B. was not satisfied with the labour of teaching mathematics and the preparation of the Polytechnic illustrative paintings. He was warmly engaged in the consideration of social and economical questions, and he was frequently employed in discovering solutions to the difficult problems which they suggested.

Some days after the revolution of February 1847, he was met by a body of workmen, among whom were some masons, who recognised him as one who had worked along with them. They immediately surrounded him, and proposed to him to become their president. After some hesitation he at length accepted the proposal. From this time he never ceased to explain to these men the social questions which were the order of the day, and to endeavour to give to these questions the solutions, of which he believed them to be susceptible. His ardour in instructing these men by speaking was so great that he lost his voice.. It was in consequence of these functions that he was chosen as delegate of the workmen at the Luxembourg.

After the revolution of February 1847, M. B. gave up his lessons and pupils, in order that he might more perfectly fulfil the mission which he had accepted; and his life was spent in a continual state of agitation and excitement. The period of the elections came; and in one of these he seized and carried off a ballot-box. For this act he was imprisoned. Of this transaction he gives the following explanation. He was following, with all the interest of a good citizen, the operations of the election, when he perceived in a corner a ballot-box which had been forgotten there. He deemed it his duty to take possession of this box in order to deposit it in a place of security, that evil-intentioned persons might be prevented from using it; but he declares expressly that the box was empty, and contained no ballot-papers.

Some time after this, he had a very animated discussion with one of the colleagues of the Mayor of the Eleventh Arrondissement, on the new elections. He had gone to the Mayor-house of his own accord, to be present at the counting of the votes. In order to justify his presence here, he alleges that he was an ancient delegate of the working-men of the Luxembourg. He was again placed under arrest.

This second arrest gave rise, like the first, to a new medical examination, the result of which appears to have been, that M. B. was in a state of exaltation of mind, which though not amounting to madness, could not be regarded as reason.

M. B. was set at liberty, and was present at the meeting on the 15th of June, at which was read the letter of Prince Louis Napoleon. Persuaded that the President of the National Assembly was following a bad course, and that he did not give a proper explanation of the thought of the letter, which according to him was given in a very satisfactory manner in the proclamation of the Prince to the electors, he descended immediately from the Tribune in which he was, in order to ask for paper. Being unable to obtain paper, he tore a leaf from his account-book and wrote on it some words. As this proceeding was interpreted into an outrage on the President of the National Assembly, M. B. was placed under arrest. The case then became a subject of judicial inquiry. M. Ch.-Auguste Lacaille, judge for the department of the Seine, issued an ordinance dated 20th June 1848, directing M. Brierre de Boismont and M. Boys de Loury, as Commissioners, to ascertain, and report on, the mental condition of M. Auguste Isaac B., Professor of Mathematics, accused of an outrage on the President of the National Assembly.

By virtue of this warrant, the Commissioners proceeded, on the 22d of June, to the place in which the prisoner was confined (the Conciergerie), and the following is the account which they gave of his demeanour and conversation.

When M. B. appeared before the Commissioners, perceiving one of them whom he knew, he cried out, " Ah! always the same accusation of madness. Madame is at the bottom of this!" He then added, that he had not forgotten the kind attentions which M. Brierre de Boismont had paid to one of his relatives, who had died in a state of mental aberration in the establishment of this physician, and that he was satisfied with seeing that the same physician had now the duty of examining him.

The reporters then requested M. Auguste Isaac B. to give them some information upon the antecedents and upon the motives which had led to his imprisonment. With this request he readily complied, and the result has been already given in the short statement above made.

M. B. is in appearance a large strong man, of sanguine temperament, of mobile physiognomy, with features indicating goodness and benevolence. He expresses himself with facility; but the abundance of his ideas prevents him from stating them with precision; and the facts are not, in all instances, methodically arranged in his head.

He maintains that he had no intention of insulting the President; that he was carried along by the extraordinary emotion which he felt, and that he regretted the expressions which he employed.

During the whole course of the conversation, which was continued more than one hour, the reporters remarked no symptom of mental derangement (*la folie*), no incoherent words, no eccentric gestures; but M. B. himself acknowledges, that the isolated position in which he is placed, by removing him from habitual causes of excitement, has rendered him more tranquil, and disposed him to express himself with less warmth.

M. B. protests, with all his might, against the imputation of madness, under the pretext of which he is confined. He is anxious indeed to submit himself to examination. The reporters interrogated him whether, in the moments in which he was placed in exceptional circumstances, of speeches, of transactions capable of producing a strong impression, he was not carried away, roused, and over-excited? He answered, that he had sacrificed his life for the public good; that he felt strongly, and could adopt an energetic resolution; but that his mental faculties were by no means disturbed.

After leaving him, the Commissioners interrogated the keepers regarding the manner in which M. B. conducted himself, and on the opinion which they had formed of him. The keepers stated that he very often spoke to himself in a loud voice; that he frequently gesticulated; and that he was generally regarded as having something singular. Others did not hesitate to assert that he was mad.

On the 5th of July, the Commissioners again went to the Conciergerie, and made a further examination. His conversation at first turned upon the events which had just been accomplished. M. B. informed the Commissioners, that if he, M. B., had been better understood, he would not have been imprisoned; that his letter to the President was merely to prevent the occurrence of a collision similar to that which had just taken place. He expressed his regret that he had been kept under bolts and locks, because he would have employed all his influence over the workmen, whose delegate he was, and the number of whom amounted to nearly ninety thousand, in order to prevent the deplorable events which had deluged the capital with blood, and that he had the conviction that he could have done some good.

The Commissioners avoided entering upon any explanation which might have led M. B. to state his ideas upon the Socialist doctrines; because, though he professed to hold himself reserved, he desired nothing so much as to speak; and they found him more irritable than formerly, incessantly giving interruptions, not completing any sentence, making frequent digressions. The point which was nearest his heart was the commission. He protested with all his energy against the imputation of madness; declaring that, in our point of view, we might draw this conclusion, because appearances gave to some of his actions a shade of caprice, but that against this judgment he protested; that the law was radically vicious, and that he would attack it by all the means in his power.

M. B. declared to the Commissioners, that an oral examination was to him of no value; that it was requisite that the questions and the answers should be in writing, and that he would approve of them, or at least authenticate their accuracy by his signature. As he was very punctilious, as he appeared ready to get into a fit of anger, as a single word might make him get beyond all bounds, they promised that they should comply with his wishes.

The interrogatories proceeded next upon the employment of his time. He told the Commissioners that he spent it in working, and that he had prepared lessons for his companions in misfortune. "My strength," he added, "is very great; I can give twelve, even fifteen hours to labour, without resting. Would you wish me to shew you my *biceps* muscle to give you an idea of my vigour?" And he uncovered himself to place it before the Commissioners.

To the questions proposed to him upon his plans in the event of his being set at liberty, he answered, that he was satisfied that it would be requisite for him to renounce all attention to social questions, and the amendment of the condition of the working-men, to whom he had hitherto consecrated all his time; that he would resume his former position in society, and would give lessons on mathematics. "I have upon this point no difficulty. The workmen, grateful for what I have done for them, have promised that they would unite in companies of thirty or forty persons, and give me each two sous for each lesson."

Recurring to the question of madness, he ascribed to the Commissioners his disappointment in not receiving the reports of the physicians to discuss them, to attack them, or to express his approbation. "I well know," said he, "that your habit of living with the insane permits you to establish your judgment without being obliged to take the precautions of a judge of instruction. But this is not my case." As he pressed the Commissioners with questions, one of them said, "Since you insist upon this, I shall tell you candidly my opinion. In all instances when you will be present at scenes favourable to emotion, you will be liable to commit some act, to utter some speech, which will cause you to be considered at least an excited person." His countenance became animated, his complexion flushed, and he answered: "Upon this point we shall never understand each other." Seeing him in this disposition, they changed the conversation. He immediately became calm; as happens to the insane when diverted from their peculiar fancies.

In this second interview M. B. was petulant, loquacious, prone to interrupt; he frequently made apologies for his conduct. His thoughts pressed upon each other and came into collision in his head. He suddenly abandoned the one which he was explaining and entered into a digression, which he speedily left to enter on another subject. If any one addressed to him observations on this point, he maintained that these digressions were the consequences of the principal idea, and he then endeavoured to establish the mutual correlation of these different ideas. When the Commissioners were preparing to take leave, he reminded them that he wished that the next examination should be conducted in the form of a judicial inquiry.

On the 10th of July 1848, the Commissioners paid him a third visit. According to his wishes they proceeded to write the questions proposed, and the answers given. They naturally proposed questions upon the causes of the arrests.

Question.—How do you explain the removal of the ballot-box?

Answer.—I will not answer this question, which I consider as judicial and not medical; I refer you to the documents. I was interrogated at this time by physicians who considered me sound in mind. He gave the same answers in relation to the discussion with the Assessor of the Eleventh Arrondissement, and his letter to the President of the National Assembly.

Q.—Were you not in a state of excessive excitement when you thus acted?

A.—I have not feared to fulfil any duty which I regard as conscientious. In certain circumstances, when I behold danger to the public welfare, I feel myself capable of all sorts of devotedness. If I had been free on the 23d of June, I would have been before the barricades in order to prevent the effusion of blood. At this moment I am no longer the delegate of the masons. I withdraw into private life.

Q.—Allow us to ask, how, having an honourable position, which afforded you a good competency, you were capable of quitting it and entering upon a career so new to you, thus risking your future subsistence and that of your wife? We will not conceal from you, that when we see a man conduct himself in this manner, we are always led to believe that his reason is no longer in a state of integrity.

A.—This is an estimate which it is not your province to make. I devoted myself to be useful to my country. I wished to direct the working-men in the right way. This I did. One man finds it natural that he should sacrifice his life on a barricade; yet people think it singular that I should sacrifice my position in society, my future subsistence to the welfare of mankind. If you wish to know a motive; I am a Jew; before '89 I was nothing. France emancipated me, with all my co-religionists. To France I owe everything; to France I will give everything. At no time have I been directed by ambition. If I had desired it, I might, thanks to my superior knowledge and attainments, have arrived at the highest employments. But my mission was entirely one of devotedness to the public good. I have asked nothing; I shall never ask anything.

Q.—Have you not had, before marriage, several attacks of cerebral disease?

A.—Why do you put this question? Unquestionably, my wife has put you on this line of inquiry. Yes; I had four attacks of cerebral fever, in consequence of considerable labours. But since 1835 I have experienced nothing of the kind, notwithstanding the trials to which I have been subjected. What inferences can any one deduce from this? M. Professor B. has had a disease of this kind; he has been removed from none of his situations, and he has been even intrusted with the re-impression of the works of La Place.

Q.—You speak of attacks of cerebral fever; but we learn that one of these was sufficiently severe to require that you should be treated in a special establishment; (a lunatic asylum).

A.—I have no occasion to answer this question. I will acknowledge, however, that I was taken to the establishment of Dole, devoted to persons alienated in mind; but it was for an ardent fever, complicated with boils, and induced by excessive labour, to which I had devoted myself in the analyses of minerals.

Q.—You previously said that you wished to return to your former profession, and to quit political life. Do you still retain the same intentions?

A.—I dislike this inquisitorial form of inquiry; I do not recognise your right thus to question me. I will tell you, nevertheless, that I adhere to my first resolutions, and that it is my wish to live in tranquillity. As to other matters, I inform you that there is enough of them. I will give no more answers. This mode of procedure appears to me illegal, false; I will no longer submit to it. I know, he added, that they wish to make me pass for a madman. My companions in captivity entreat me strongly to lay hold of this anchor of safety. Many of my friends believe that I am at least singular. But such a means of defence I cannot accept. I have maintained those principles which alone I believe to be true, to be just. These principles I desire to proclaim, and to defend in the face of tribunals. Lastly, I am proud of the glory which will, in after ages, gild my name, when posterity shall render me justice, and will, with painful astonishment, ask the question, How the author of ideas so useful could have been considered as a madman?

The Commissioners went to different persons who had known M. B., and particularly to the superintendent of an institution in which he had taught for several years. They all agreed in representing M. B. to be a man of worth, good, animated by the best intentions, but also as irritable, exalted, proud, and often speaking of the intention that he had of becoming Minister of Public Works, in order to carry his schemes into execution.

Meanwhile, upon making a summary of the facts contained in their report, the Commissioners think, that they are entitled to establish the following inferences upon the case of M. B.

1. M. B., without being exactly insane in the ordinary acceptation of this term, exaggerates everything that he does. By the nature of his organisation, sanguine and irritable, he is impelled, in spite of himself, to express his thoughts by actions, capricious, singular, eccentric.

2. M. B. belongs to that numerous class of persons, original, haughty, who at every instant depart from the ordinary line,

are seized with enthusiasm for all systems, are continually in movement, form a thousand projects, proclaim themselves as benefactors of humanity, regenerators of society, dissipate their means, carry nothing to a good termination, and which may be considered as the earliest form or stage of insanity.

3. The antecedents of M. B., who by his own acknowledgment, has had four times an attack of cerebral fever, and has been once confined in an asylum for the insane, shew that he has a predisposition to insanity, and give at least ground to presume, that he has a modification in his intellectual faculties.

4. The facility with which he becomes animated, irritated even, when there is proposed to him a question which he dislikes, permits us not to entertain any doubt, that he was in the same state of over-excitement at the time of the facts alleged, and especially at the time of his letter to the President of the National Assembly.

5. The kind of life which M. B. has been leading, being constantly in the midst of workmen, incessantly speaking to them, haranguing them, presiding at their clubs, discussing their interests at the Luxembourg, has had no other effect than that of increasing his natural state of excitability, and developing in his system a genuine state of fever.

6. The excellence of his character may be a warrant as to the gravity of his actions; but it is almost certain, that whenever he is placed under the influence of any state of excitation, he will act as he has done on the three occasions on which it was necessary to place him under arrest.

7. From these considerations, we think that M. B. was not in the possession of his faculties, in a state of integrity, at the time at which he addressed the note to the President of the National Assembly. There is doubtless at present an amendment in his state; but he has not yet returned to form a correct estimate of his words and actions.

He may be placed at liberty. Nevertheless, it is to be feared, that on the first occasion, he will commit some unreasonable act; it would be then prudent to keep him for a time, shorter or longer, in a special establishment; (a lunatic asylum).

M. B. was set at liberty. The Commissioners often meet him. He has renounced his theories; he is no longer occupied with the subject of social improvement. He has resumed his didactic labours, and may yet be a useful member of society, if he persevere in this good course.

There are in all societies, and all large towns and cities at the present time, not a few duplicates of M. B. We have no doubt that many practical physicians will recognise one or more of these duplicates, not perhaps exact copies, for that is almost impossible, but so closely resembling the model, that it would be waste of time to attempt a formal demonstration of the similitude.

What are the distinguishing characters of this mental disorder? Is it possible to describe and define the exact lesion of the mental constitution which takes place in this class of individuals? We are aware of the difficulty of this undertaking, and hence we are diffident as to the possibility of giving a just and comprehensive definition or description. The following remarks therefore are submitted with that degree of diffidence which the difficulty of the subject renders becoming.

In these persons several of the moral sentiments and emotions, seem to be as it were in a condition of exaggeration, and either in, or liable to assume, a state of disproportionate action and energy to that of others. The emotions which elevate,—as vanity, self-esteem, conceit, confidence,—are all in exaggeration. The swelling effect as it were, to use the language of material objects, makes the individuals take partial views, or look only at the favourable side of things, and make no reckoning of difficulties, obstacles, and natural impediments. They not only overrate the influence of their own powers, but they underrate the opposition and resistance which they are almost certain to meet. There is enthusiasm, but there is something more. Enthusiasm and energy, under the guidance of judgment, enable men to perform numerous enterprizes which to all others seem impossible. Columbus was a man of enthusiasm, energy, and self-reliance, when he discovered the Antilles and the Continent of America; Luther was a man of enthusiasm and energy, when he preached against indulgences, the infallibility of the Pope, and justification by works; but though the latter was a very credulous, superstitious person, neither were disordered in mind. Judgment and deliberation in both instances kept them in the right path. In persons, however, of the kind here under consideration, either judgment and reflection do not exist, or they are prevented from exerting any influence by the greater vigour of the moral emotions.

It is observed further in all the instances of this form of mental disorder, that the thoughts are confused, and that the ideas rise up in such abundance and disorder that the individuals have no power of arranging them methodically. They have no power of controlling trains of thought. This peculiarity has been often remarked by common observers in many persons, who are then said to have a confused manner. In general these persons are great talkers; but there is in their speaking and discoursing little order, almost no precision of ideas. Association or suggestion seems to be very active; but not being placed under the control of reflection, its operations are rather hurtful than beneficial.

ART. VII.—*Observations on the Generation of Uric Acid, and its Crystalline Forms.* By S. WEIR MITCHELL, M.D. (American Journal of Medical Science, No. xlvii., New Series, July 1852, p. 121.)

The part performed by uric acid in the healthy urinary secretions is so important, and its relations with the morbid states of that secretion so interesting, that it has been looked at with great attention, and lately studied in every point of view.

Deeply connected with its therapeutics, and of kindred interest, are its numerous and beautiful crystalline forms, surpassing in elegance of figure, in colour and in variety, all other urinary crystals.

These forms of uric acid vary so greatly, that it would naturally be supposed that their distinctions of shape depend upon perceptible varieties in the chain of healthy or morbid processes which give rise to their formation, and that, by careful study, we might be able to trace out such relations, and make use of them for therapeutical and nice diagnostic purposes. Thus far, however, such has not been the case; and although some such alliances of cause and effect are in the highest degree probable, we have not been so fortunate as to elucidate them to any very valuable or available extent.

We are aware of the change from rhombs to squares, which occurs in the progress of old cases of lithuria; and we are also aware of the fact that a tendency to aggregate in masses, as seen without the bladder, is only too indicative of a similar disposition towards aggregation within it, and, consequently, to the formation of calculus.

At present, I would desire to call attention to a point in the crystallography of uric acid, which has attracted considerable attention, and given rise to several explanations. These views are mainly theoretic, and are unsupported by what may be termed crystalline analysis, on which I have chiefly relied in obtaining the results detailed in this paper.

According to Golding Bird, all the varieties of form, so constantly met with in uric acid, are referable to the rhombic prism. It is singular that these alone, of all the forms, we have not been able to reproduce, out of the body, by the addition of an acid to urine. Dr Bird very properly states that the beautiful *fleurs-de-lis* crystals, which he has figured, are cubes or rhomboids, with indented edges. I have sometimes obtained these so slightly indented as to make them look almost identical with the primitive form.

Another and a very frequent variety, of crystalline shape, is the lozenge, found very often in natural deposits, and almost invariably present in the sediment of uric acid, caused by the addition of a *few drops* of hydrochloric acid to a large quantity of urine. When these lozenges are very thin, they present to the observer their elliptical faces ; but when of greater thickness, they offer the appearance of cylinders, brilliantly lighted in the middle, and shaded at the two extremities. At times, and in a few specimens, two lozenges appear to have formed at right angles to one another, and to have become perfectly united during the process of formation. Such crystals offer a very beautiful starlike form, varying in thickness and in size, like the lozenges from which they derive their existence. This variety closely resembles the *fleur-de-lis* crystals. I have sometimes noticed, both in these and in the simple lozenge, a long spine, like an acute pyramid, projecting from one of the extremities of the lozenge, and resembling a marking of the same shape as that which is often seen upon the surface of the lozenges, and which has been already described and drawn by Bird.

The last variety to which I shall now allude, is the oblong plate, whose peculiar and beautiful markings have attracted so much attention, and received so many explanations. Before referring to this, I wish, however, to describe some similar and equally curious markings, which are, I believe, undescribed, and which belong to the lozenge-crystals above mentioned. I hope, by these means, to explain satisfactorily the crescentic curvatures of the tabular crystals, and to cast some light upon the primary forms of uric acid deposits.

When a well-formed lozenge is placed upon its side so as to present to the eye the appearance of an oblong figure, two distinct and very perfect crescentic marks are seen commonly touching one another at their convexities, and including within their curves the greater mass of the crystal to which they belong. These markings rarely shew well in such crystals as belong to natural deposits, and in most cases are entirely invisible, or to be seen only after long digestion in Canada balsam.

Having been much struck by this resemblance to the curves on the faces of the tabular variety, I have been at some pains to ascertain the cause of the likeness. After numerous experiments, I believe I am able to explain these, as well as some other peculiarities of uric acid deposits. The very small size of the bodies with which we have to deal, as well as the necessity of conducting our researches under the microscope, renders it impossible to avail ourselves of the methods applicable in ordinary crystallography, and I was therefore obliged to use certain agents not generally employed.

In the course of these studies, I placed in each of four test-tubes about half an ounce of urine. To the first of these, I added one drop of hydrochloric acid, to the second, five drops; and to the third and fourth, ten and thirty drops respectively. At the end of thirteen hours, I examined the deposits in all the tubes. No. 1 presented a few very minute lozenges, pale, thin, and in many cases looking like little spheroids. Nos. 2 and 3 contained larger and thicker lozenges, while No. 4 presented, not only large lozenges, but a few well-formed tabular crystals. I now removed a drop of fluid from No. 1, and placed it on a slide to dry. As evaporation went on, the acid naturally became more concentrated, and when, after drying it, I moistened the plate with water, and placed it under the microscope, I was surprised to see the field covered with singular little crystals. These were shaped precisely like an hour-glass, or like two half spheres, placed with their convexities in contact. The outlines of these bodies resembled precisely the marking on the large lozenge-crystals, and although varying a little in their degrees of curvature, were of the same type throughout. I next observed that some of them had the space between the two half spheres filled more or less by what I have since concluded to be minute acicular crystals. In a few, this space was completely occupied, and the general form was then seen to be a lozenge, with curves due to its primitive character, and with elliptical surfaces upon the two ends. In the course of time, most of the crystals underwent a like change, and the hour-glass form disappeared altogether. I have retained some of these crystals, but I have not yet been able to reproduce others of a like character, although I have made many efforts.

I next placed a few well-formed lozenges under the glass, and, adding a drop of diluted nitric acid, watched the process of solution. In a few moments, the acicular structure between the two curves began to give way, being attacked most easily on account of its loose composition. While it melted rapidly, the denser primitive forms remained untouched, and, finally, the crystals presented a beautiful hour-glass shape, encircled in some cases at the apices of the primitive forms by a black band. This appearance was due to the irregular and uneven surface of the yielding fibrous structure, which refracted the light so as to appear black. In others, the hour-glass was left naked, and, in some, the corners were rounded off by the continued action of the acid.

In my final experiment, I placed some lozenges of uric acid in water, and heated them slowly. On placing these beneath the microscope, the surfaces which represent oblongs were seen to be marked with dark lines shewing very beautifully the form of the acicular crystals as they lay agminated about the primitive nuclei. I do not rely greatly on this appearance, since I have not found it uniform, the crystals often becoming striated in the other direction. It will thus be seen that little doubt is left as to the origin of the curves on the lozenges, and I believe I can shew that the tables owe their marks to a like cause. From repeated observation, I have come to the conclusion that they are in fact large lozenges, of no great thickness, but having an oblong or square surface, on which are seen the marks of their primitive form, and around which the acicular crystals have collected, clothing it completely. Upon either extremity, a cluster of such needles has formed perpendicularly to the elliptical face of the crystal, and can often be broken off in mass from the main body.

I have thus obtained a knowledge of the form of these crystals by synthesis, by analysis, and by the aid of heat.

On reference to *Bird on Urinary Deposits*, it will be seen that he suspected these crystals to be compound, and indeed suggested what he believed to be their ultimate form, which he referred to the union of two acute rhombs superimposed laterally. He also states that Dr Burton, of Walsall, ascribes them to the union of two rhomboidal (?) figures, surrounded by acicular crystals ; an opinion which is very nearly correct, but for which he gives no reasons. However arrived at, this view is very ingenious, and very close to the truth. Neither of these gentlemen seems to have noticed the curves on the lozenges, which gave the key to those upon the tabular variety.

It is highly probable, that observation of some of these curious hour-glass forms, misled Dr Fricke of Baltimore into the belief that the dumb-bell crystals of oxalate of lime were forms of uric acid, with which they have, in reality, very little analogy. In the early part of this paper, I have alluded to a stellar form of uric acid, the result of the conjoined growth of two lozenges placed at right angles to one another. I may add, that they sometimes exhibit beautiful markings when rolled on their sides, and even shew the indentations resulting from the ultimate forms of their component lozenges.

There are some other remarks in connection with the crystallography of the urine which I would wish to add, but I reserve them for a future communication.

ART. VIII.—*Analysis of the Water of the Thermal Spring of Bath (King's Bath).* By Messrs GEORGE MERCK and ROBERT GALLOWAY. (Memoirs and Proceedings of the Chemical Society of London, vol. iii. London, 1848, p. 262.)

[Dr Tunstall, in his work on the Bath Waters, it was mentioned in last Number, gives, as the most recent, the analysis of these mineral waters by Mr Noad. It appears not to be known to that gentleman that an analysis has been given subsequently to that of Mr Noad, by Messrs Merck and Galloway in the Quarterly Journal of the Chemical Society. This we thought of mentioning at the time, when we spoke of the mineral constituents of these waters; but we found, upon trial, that to notice the analysis of Messrs Merck and Galloway so as to do justice to these chemists, and to be of any service to our readers, it would have occupied too much space, and impaired the unity of the article on the work of Dr Tunstall. Upon reflection, therefore, it appeared most eligible to give this analysis of Messrs Merck and Galloway separately; and we accordingly now introduce it in this place.

The comparative observations which these chemists have made upon the labours of previous chemists, render it unnecessary here to add other observations. It will be seen that the results obtained by Messrs Merck and Galloway approach most nearly to those obtained by Mr Walker. It will be observed also, that sulphate of lime still figures as a large ingredient, at least in the results. Whether it does so in the actual water, we feel that it would not be becoming in us to say more than we did in last Number. We desire, therefore, now to leave to the judgment of our readers the subject, and the question of the mineral and saline agents which exert the greatest physiological influence and therapeutic effects upon the human body.]

The water of this celebrated spring, the efficacy of which was known in the time of the Romans, has been analysed repeatedly by various chemists at different periods. Richard

Phillips,[*] Scudamore,[†] Walker,[‡] and more recently Noad,[§] have occupied themselves in the investigation of this water. In their several analyses, the whole amount of the fixed ingredients of the water agrees very closely; but in regard to the composition of these substances, there are considerable discrepancies, as may be seen in a table which we have annexed at the end of this paper.

Besides great differences in the quantitative analysis, we find discrepancies even in regard to the presence and absence of certain constituents. Among the chemists that have been mentioned, Walker is the only one who has recognised the presence of potash. The same chemist corroborated Scudamore's statement as to the presence of magnesia, overlooked by their predecessors; but he states also that he detected alumina, which none of the others found. In all these analyses, iodine has been omitted. Mr Cuff,[||] however, has indicated the presence of this element in the spring.

These discrepancies made another investigation of the mineral water of Bath very desirable; the following analysis was performed at the suggestion of Dr A. W. Hofmann.

To obtain the water genuine, and especially for the purpose of ascertaining the amount of free carbonic acid it contained, we collected the water ourselves, an operation in which we were kindly assisted by Messrs Green and Simms, lessees of the establishment

The water was taken from the principal well, which supplies the King's and Queen's baths, which are the most esteemed and valued in the city. Of the two other wells, one supplies the Hot Bath and the other the Cross Bath, which are in the neighbourhood of those first mentioned.

The King's Bath is an oblong cistern, 65 feet long and 40 feet broad, in which the water stands at the height of 46 inches. It is supplied from the bottom by means of twelve large and about twenty smaller apertures. By far the largest amount of water rises however from an opening made in the centre of the bath, 18 inches in diameter. Although the water flows under the influence of a very small pressure, the quantity is such, that the two reservoirs, the

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[*] An Analysis of the Bath Water. By Richard Phillips. London, 1806.

[†] A Chemical and Medical Report of the Properties of the Mineral Water of Buxton, Matlock, &c. By Ch. Scudamore, M.D. 1820.

[‡] Quarterly Journal of Science, Literature, and Arts, vol. xxvii. 78. 1829.

[§] Pharmaceutical Journal, vol. iii., 526.

[||] Memoir on the occurrence of Iodine and Bromine in certain Mineral Waters of South Britain. By Charles Daubeny; Transactions of the Royal Society of London, 1830; ii., p. 223.

King's and the Queen's Bath, are entirely filled in about nine hours. The quantity of water entering each minute is 126 gallons, upon the authority of Dr Daubeny.*

I. *Qualitative Analysis.*

The water as it issues from the well has a temperature of 46° C. (115° Fahr.), the temperature of the air being 20° C. (68° Fahr.); it is clear and without odour, and has no effect upon vegetable colours; it has a saline and slight iron taste; the iron is deposited as sesquioxide in rather large quantities in the pipes leading from the well.

The following experiments gave the qualitative composition of the mineral water; on boiling for some time, a white crystalline precipitate formed. The qualitative analysis was therefore divided into two parts.

a. The analysis of the precipitate formed on boiling.

b. The analysis of the substances remaining dissolved.

a. Analysis of the Precipitate formed on Boiling.

1. The precipitate was treated with hydrochloric acid; a small portion of it dissolved with effervescence, indicating the presence of carbonic acid. The portion insoluble in hydrochloric acid dissolved on the addition of a large quantity of water,—*indicating sulphate of lime.*

Another portion of the water was boiled some time, with the precaution of replacing the evaporated water, in order that all the sulphate of lime should remain in solution; in this case only a very small precipitate was formed, which was entirely soluble in hydrochloric acid.

2. On heating this solution and adding ammonia, a very slight flocculent precipitate of a yellowish-white colour was produced after some time,—*indicating oxide of iron.*

3. In the filtrate from the sesquioxide of iron (2.), on the addition of oxalate of ammonia, a white precipitate was formed,—*indicating salts of lime.*

4. In the liquid filtered off from the oxalate of lime (3.), phosphate of soda produced an exceedingly slight crystalline precipitate,—*shewing the presence of magnesia.*

Note.—This precipitate could only be distinctly seen in testing a large quantity of the water.

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* On the Quantity and Quality of the Gases disengaged from the Thermal Spring which supplies the King's Bath in the City of Bath. By Charles Daubeny; Transactions of the Royal Society of London, 1834, i., 1.

b. Analysis of the Substances remaining dissolved.

The liquid which was filtered from the precipitate (*a.*) formed on boiling had no alkaline reaction; a portion of it was evaporated nearly to dryness, and treated with hydrochloric acid; no carbonic acid was evolved, from which comportment the absence of alkaline carbonates could with safety be concluded.

1. A portion of the liquid gave, on addition of chloride of barium, a copious white precipitate, insoluble in hydrochloric acid,—*indicating sulphuric acid.*

2. In another portion of the liquid, nitrate of silver produced a copious white precipitate, easily soluble in ammonia, —*evidencing the presence of chlorine.*

3. The entire solubility of the silver precipitate seemed to indicate the absence of iodides. To make ourselves perfectly certain of the absence of these salts, thirty or forty pounds of the water were evaporated to two or three pounds, and the liquid filtered off from the precipitate which had been formed; a part of this fluid was evaporated with precaution to dryness, the residue was mixed with some starch paste, and a few drops of nitric acid being added, feeble but distinct violet spots were observed. This experiment was repeated several times with the same success,—*indicating the presence of iodine.*

4. Another portion of the liquid (*b.*) was treated with hydrochloric acid, evaporated to dryness, and gently ignited; on treating the residue with a large quantity of water, an insoluble portion remained,—*shewing the presence of silicic acid.*

5. Another portion of the liquid (*b.*) gave, on addition of chloride of ammonium and oxalate of ammonia, a white precipitate,—*indicating lime.*

6. On adding, to a portion of the filtrate, ammonia and phosphate of soda, a slight crystalline precipitate was formed, —*indicating magnesia.*

7. For the discovery of the alkalies, the remaining portion of the filtrate from the lime precipitate was evaporated to dryness, and the residue ignited until the ammoniacal salts had been expelled. The ignited residue was then dissolved in water, the sulphuric acid and magnesia precipitated by baryta water, and, after separation of the excess of baryta by means of carbonate of ammonia, the filtrate evaporated to dryness and ignited. The residue imparted a yellow colour to the blow-pipe flame,—*evidencing the presence of soda.*

An alcoholic solution of the residue gave, with a concentrated solution of bichloride of platinum, a yellow crystalline precipitate,—*indicating potassa.*

The precipitate which had formed on evaporating for the iodine determination, was treated with hydrochloric acid, the filtrate saturated with ammonia, and precipitated by sulphide of ammonium; this precipitate was re-dissolved in nitro-hydrochloric acid mixed with chloride of ammonium, and the sesquioxide of iron separated by ammonia. The filtrate, evaporated and fused with nitrate of potash and carbonate of soda, gave a green mass,—*shewing traces of manganese.*

Lithia, alumina, bromine, and phosphoric acid, were found to be absent.

In regard to the presence of gases in the water, it was scarcely necessary to test for the presence of free carbonic acid. On mixing a solution of lime with the mineral water, a precipitate was formed, which dissolved in an excess of the mineral water. The quantity of free carbonic acid, however, is not very large; the water has no reaction on blue vegetable colours; hydrosulphuric acid is not contained in the water. Acetate of lead gave only a white precipitate of sulphate of lead free from all trace of brown colour, which might indicate the presence of sulphur.

A large quantity of gas is continually disengaged from the chief spring as well as from the secondary ones. Dr Daubeny* paid particular attention to the composition of this gas. He found that it consists principally of nitrogen, together with small quantities of carbonic acid and oxygen.

He employed a peculiar apparatus, constructed on purpose for these experiments, by which he was enabled to collect the whole of the gases from the principal well, as well as from those adjoining it. The experiments of Daubeny are so numerous and accurate, as to preclude any other researches on the subject.

II. *Quantitative Analysis.*

Determination of the Specific Gravity.

A small bottle, which contained at the temperature of 16·5° C. (60° Fahr.) 10 grms. of distilled water, contained at the same temperature 10·025 grms. of the mineral water; from this the specific gravity of the water is calculated as 1·0025.

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* Vide Memoir mentioned.

1. *Estimation of Sulphuric Acid.*

The mineral water was heated with a little hydrochloric acid and chloride of barium added.

I. 534·199 grms. of water gave 1·340 grm. of sulphate of baryta = 0·4605 grm., or 0 08620 per cent. of sulphuric acid.

II. 475·003 grms. of water gave 1·1791 grm. of sulphate of baryta = 0·4050 grm., or 0·08526 per cent. of sulphuric acid.

Mean of the results, 0·08573 per cent.

2. *Estimation of Chlorine.*

The water was treated with nitric acid and precipitated by nitrate of silver; the precipitated chloride of silver was washed by decantation, fused and weighed.

I. 101 grms. of water gave 0·1137 grm. of chloride of silver = 0·2811 grm., or 0·02778 per cent. of chlorine.

II. 100·006 grms. of water gave 0·1093 grms. of chloride of silver = 0·02702 grms., or 0·02701 per cent. of chlorine.

Mean of the results, 0·02739 per cent.

3. *Estimation of Silicic Acid.*

To the water was added nitric acid in excess; it was then evaporated to dryness and the residue for some time heated on the sand-bath. On treating this residue with water and hydrochloric acid, the silicic acid remained behind; it was collected, washed, and weighed.

I. 765·325 grms. of the water gave 0·0342 grm., or 0·00446 per cent. of silicic acid.

II. 732·015 grms. of water gave 0·0289 grm., or 0·00407 per cent. of silicic acid.

Mean of the results, 0·00426 per cent.

4. *Estimation of Iron.*

The iron was estimated,—

(A.) In the precipitate formed on boiling the mineral water.

(B.) In the water which had not been boiled.

Both estimations gave the same results.

(A.) Estimation of the iron in the precipitate.

A certain quantity of the water was boiled for some time; the precipitate which had formed was washed, dissolved in hydrochloric acid and precipitated by an excess of ammonia.

I. 777·215 grms. of water gave 0·0079 grm., or 0·00101 per cent. of sesquioxide of iron.

(B.) Estimation of the iron in the water which had not been boiled.

The liquid filtered off from the silicic acid (3.) was concentrated and precipitated by an excess of ammonia.

II. 765·325 grms. of water gave 0·0078 grm., or 0·00101 per cent. of sesquioxide of iron.

III. 732·015 grms. of water gave 0·0086 grm., or 0·00116 per cent. of sesquioxide of iron.

Mean of the results, 0·00106 per cent., corresponding to 0·00153 per cent. of carbonate of oxide of iron.

5. *Estimation of Lime.*

The estimation of the lime was divided into—

(A.) Estimation of the lime contained in the water in the state of carbonate.

(B.) Estimation of the lime contained in the water in the state of sulphate.

(C.) Estimation of the total amount of lime for control.

(A.) Estimation of the lime combined with carbonic acid.

The ammoniacal liquor filtered off from the precipitate of sesquioxide of iron was precipitated by oxalate of ammonia; the oxalate of lime was converted in the known way into carbonate.

I. 712·747 grms. of water gave, on boiling, a precipitate containing 0·0904 grm. of carbonate of lime = 0·05062 grm., or 0·00712 per cent. of lime.

II. 623·881 grms. of water gave, on boiling, a precipitate containing 0·0782 grm. of carbonate of lime = 0·0437 grm., or 0·00700 per cent. of lime.

Mean of the results, 0·00706 per cent.

(B.) Estimation of the lime combined with sulphuric acid.

The mineral water was kept boiling for one or two hours, replacing the water which evaporated; the precipitate formed was filtered off, washed, and to the filtrate was added chloride of ammonium, ammonia, and oxalate of ammonia; the oxalate of lime was converted into carbonate.

I. 710·747 grms. of water gave in this way 0·6072 grm. of carbonate of lime = 0·3400 grm., or 0·04783 per cent. of lime.

II. 623·881 grms. of water gave 0·5165 grm. of carbonate of lime = 0·2892 grm., or 0·04635 per cent. of lime.

Mean of the results, 0·04709 per cent.

(C.) Estimation of the total amount of lime for control.

The ammoniacal liquid which was filtered off from the precipitate of sesquioxide of iron was precipitated after the addition of chloride of ammonium by oxalate of ammonia, and the oxalate of lime converted into carbonate.

I. 765·325 grms. of water gave 0·7211 grm. of carbonate of lime = 0·403816 grm., or 0·05276 per cent. of lime.

II. 732·015 grms. of water gave 0·6981 grm. of carbonate of lime =0·3909 grm., or 0·05340 per cent. of lime.

Mean of the results, 0·05308 per cent.

Mean of the lime combined with carbonic acid	0·00706
Mean of the lime combined with sulphuric acid	0·04709
Total amount found by addition	0·05415
Mean of the total amount found by direct estimation	0·05308

6. *Estimation of Magnesia.*

The estimation of the magnesia was divided in the same manner as the estimation of lime into—

(A.) Estimation of the magnesia combined with carbonic acid.

(B.) Estimation of the magnesium combined with chlorine.

(C.) Estimation of the total amount of magnesia for control.

(A.) Estimation of magnesia contained in the water as carbonate.

To the liquid filtered off from the oxalate of lime was added phosphate of soda; on stirring, after some time a precipitate of phosphate of magnesia and ammonia was formed, which was converted by ignition into pyrophosphate of magnesia.

I. 777·215 grms. of water gave, on boiling, a precipitate which contained 0·0046 grm. of pyrophosphate of magnesia = 0·001685 grm., or 0·00021 per cent. of magnesia.

II. 623·881 grms. of water gave, on boiling, a precipitate which contained 0·0044 grm. of pyrophosphate of magnesia = 0·00016 grm., or 0·00025 per cent. of magnesia.

Mean of the results, 0·00023 per cent.

(B.) Estimation of the magnesia contained in the water as chloride of magnesium.

The liquid filtered off from the oxalate of lime was concentrated by evaporation, ammonia added filtered off from a small portion of silicic acid which separated, and the magnesia precipitated by phosphate of soda.

I. 414·279 grms. of water gave in this way 0·1007 grm. of pyrophosphate of magnesia = 0·03689 grm., or 0·008906 per cent. of magnesia.

II. 427·1 grms. of water gave 0·1050 grm. of pyrophosphate of magnesia = 0·03846 grm., or 0·009004 per cent. of magnesia.

Mean of the results, 0·008955 per cent.

(C.) Estimation of the total amount of magnesia for control.

The liquid filtered off from the precipitate of oxalate of lime was concentrated, ammonia and phosphate of soda added.

I. 765·325 grms. of water gave 0·1936 grm. of pyrophosphate of magnesia = 0·070929 grm., or 0·00926 per cent. of magnesia.

II. 732·015 grms. of water gave 0·1837 grm. of pyrophosphate of magnesia = 0·0673 grm., or 0·00919 per cent. of magnesia.

Mean of the results, 0·00922 per cent.

Mean of the magnesia combined with carbonic acid	0·00023
Mean of the magnesia contained in the water as chloride of magnesium . . .	0·00895
Total amount found by addition	0·00918
Mean of the total amount found by direct estimation	0·00922

7. *Estimation of the Alkalies.*

For the estimation of the alkalies, the mineral water was evaporated to one-third of its volume, and baryta water added in excess, the precipitates of sulphates of baryta, lime, magnesia, and sesquioxide of iron, were filtered off, and the excess of baryta precipitated by means of carbonate of ammonia. To get rid of the silicic acid, the filtrate was evaporated to dryness with hydrochloric acid, gently ignited, dissolved in water, again filtered and evaporated to dryness; the mixed chlorides obtained in this manner were weighed.

I. 632·481 grms. of the mineral water gave 0·2937 grm. of chloride of sodium and chloride of potassium = 0·04643 per cent. of the mixed chlorides.

II. 546·032 grms. of water gave 0·2538 grm. of chlorides of sodium and potassium = 0·04648 per cent. of the mixed chlorides.

Mean of the results, 0·04645 per cent.

8. *Estimation of the Potassa.*

The chlorides of potassium and sodium were dissolved in a small quantity of water, and an excess of bichloride of platinum added; the liquid was then evaporated to dryness in the water-bath, the residue digested with alcohol, the insoluble chloride of platinum and potassium filtered off from the soluble sodium salt, and washed with alcohol; the precipitate was dried in the water-bath and weighed.

I. 632·481 grms. of the mineral water, or 0·2987 grm. of the mixed chlorides, gave 0·124 grm. of chloride of platinum and potassium = 0·0378 grm. of chloride of potassium = 0·00597 per cent. of chloride of potassium, which equals 0·00377 per cent. of potassa.

II. 546·032 grms. of water, or 0·2538 grm. of the mixed chlorides, gave 0·0975 grm. of chloride of platinum and potassium = 0·02977 grm. of chloride of potassium = 0·00545 per cent. of chloride of potassium, which equals 0·00342 per cent. of potassa.

Mean of the results, 0·00571 per cent. of chloride of potassium and 0·0359 per cent. of potassa.

9. *Estimation of the Soda.*

The quantity of soda was found simply by the difference of the mixed chlorides and the quantity of chloride of potassium found by direct estimation.

Mean of the mixed chlorides, .	0·04645
Mean of the chloride of potassium,	0·00571
Chloride of sodium,	0·04074

corresponding to 0·02168 per cent. of soda.

10. *Estimation of Carbonic Acid.*

To find the quantity of free carbonic acid contained in the water at the moment it was taken from the well, a syphon of exactly known capacity was immersed in the well, and the water obtained in this way put in bottles, containing a mixture of ammonia and chloride of calcium. In this way the free carbonic acid, as well as the carbonic acid in combination, was precipitated in the form of carbonates. Four bottles were filled with mineral water by this method. The capacity of the syphon was exactly 533 cubic centimetres, therefore 533 × 4 × 1·0025 = 2137 grms. of water were taken.

The precipitate from the water contained in these four bottles was collected, washed, dried, and weighed; it yielded 1·4748 grm. of carbonate mixed with some alumina from impurity in the solution of chloride of calcium.

To estimate the quantity of carbonic acid in this precipitate, two portions of it were taken and estimated separately after the method proposed by Drs Fresenius and Will.

I. 0·66 grm. of the carbonate, &c. gave in this way 0·22 grm. of carbonic acid, therefore, 1·4748 grm. of the carbonate, or 2137·0 grms. of water, gave 0·4916 grm. of carbonic acid.

II. 0·718 grm. of the carbonate, &c. gave 0·23 grm. of carbonic acid, therefore 1·4748 grm. of the carbonate, &c., or 2137·0 grms. of water gave 0·4718 grm. of carbonic acid.

Mean of the results.

0·4817 grm. of carbonic acid, which equals 0·02254 per cent.

Total amount of carbonic acid, .		0·02254
Carbonic acid existing in combination—		
With oxide of iron, .	0·00057	
With lime, . .	0·00554	
With magnesia, .	0·00024	
Sum total, ...		0·00635
Free carbonic acid remaining, .		0·01619

From the details contained in the preceding pages, it follows that the thermal spring in the King's Bath contains the following constituents in 100 parts :—

Carbonate of lime, . .	0·01260
Carbonate of magnesia, .	0·00047
Carbonate of oxide of iron, .	0·00153
Sulphate of lime, . .	0·11436
Sulphate of potassa, . .	0·00663
Sulphate of soda, . .	0·02747
Chloride of sodium, . .	0·01806
Chloride of magnesium, .	0·02083
Silicic acid, . . .	0·00426
	0·20620

Traces of manganese and iodine.

Estimation of the total amount of the fixed ingredients in the water for control.

The water was concentrated in a porcelain dish, and afterwards evaporated to dryness in a platinum basin. The residue was heated in an air-bath until the weight was constant. Two estimates were made.

I. 217·058 grms. of water gave 0·4540 grm., or 0·20916 per cent. of residue.

II. 319·57 grms. of water gave 0·6726 grm., or 0·21040 per cent. of residue.

Mean of the results, 0·20978 per cent.

But in this experiment the iron was obtained in the state of sesquioxide, whilst in the preceding calculation it is taken as the carbonate of the oxide, in which form it exists in the water.

On calculating the absolute weights from the above, we obtain the following numbers.

	In a Litre.	In an Imperial Gallon. (70,000 grs.)
Carbonate of lime, .	0·1260 grm.	8·82000 grs.
Carbonate of magnesia,	0·0047 ...	0·32900 ...
Carbonate of oxide of iron,	0·0153 ...	1·07100 ...
Sulphate of lime, .	1·1436 ...	80·05200 ...
Sulphate of potassa, .	0·0663 ...	4·64100 ...
Sulphate of soda, .	0·2747 ...	19·22900 ...
Chloride of sodium, .	0·1806 ...	12·64200 ...
Chloride of magnesium, .	0·2083 ...	14·58100 ...
Silicic acid, . .	0·0426 ...	2·98200 ...
	0·20621	144·01800

According to our experiments, 1 litre of the water contains 95·64 cubic centimetres of the carbonic acid, at free temperature of 46° C. (115° F.) and normal atmospheric pressure.

One imperial gallon contains therefore 26·45 cubic inches of free carbonic acid of 46° C., being more than double the quantity which has been determined by former experiments. This, however, is not surprising, as the estimations previously made had been effected by the expulsion of the carbonic acid from the water. Besides the difficulty of avoiding a loss of carbonic acid before the operation, it is scarcely possible, as Mr Phillips justly notices in his paper, to expel all carbonic acid by simple ebullition. Besides, we see from the experiments of Daubeny, that the gas which escapes from the well contains at different periods highly-varying amounts of carbonic acid. He found by several experiments that the King's Bath evolves on an average 267 cubic inches of gas per minute, or 223 cubic feet in twenty-four hours. He further ascertained that this gas consists nearly entirely of nitrogen, mixed with a small amount of oxygen and carbonic acid, and that these gases were generally in the following proportion :—

Nitrogen . . = 91·9
Oxygen . . . = 3·8
Carbonic Acid = 4·3

In many instances, however, he observed as much as 7·4 to 8·2, and even once 11·5 parts of carbonic acid.

From these observations there is no doubt that the quantity of carbonic acid dissolved in the water is very variable.

In the following table we give the analyses of former experimenters, calculated in an imperial gallon (70,000 grs.)

	Phillips.	Scudamore.*	Walker.	Noad.
Carbonate of lime	7·680	5·280	10·667	
Carbonate of oxide of iron	0·274	0·200	0·243	0·521
Carbonate of soda	....	....	....	5·760
Sulphate of lime	86·400	98·320	81·624	96·240
Sulphate of potassa	....	....	2·927	
Sulphate of soda	14·400	1·520	19·371	
Chloride of sodium	31·680	12·240	15·122	27·456
Chloride of magnesium...	....	15·360	13·339	7·142
Alumina	....	....	0·150	
Silicic acid	1·960	1·920	3·233	3·360
	142·394	134·840	146·676	140·479
Quantity directly observed	144·125	....	147·622	149·72
Carbonic acid...............	11·52 cub. inch.	....	7·60 cub. inch.	

Our analysis agrees, as may be seen, best with that of Walker. According to Professor Liebig's† arrangement of mineral waters, the thermal spring of Bath would belong to the saline waters containing carbonic acid.

It may be not without its use, to present the figures given in these tables, according to the weights of the atomic theory. These have been reduced by careful calculation from the numbers, by a gentleman well versed in practical chemistry.

In 10·000 parts of weight.

		Atoms.	
Sulphuric acid, ...	8·573	=1·7146	
Chlorine, ...	2·739	=0·608	
Silica, ...	0·426	=0·213	Thom.
Carbonate of protoxide of iron,	0·153	=0·211	
Lime, with carbonic acid, .	0·706	=0·202	
Lime, with other acids, . .	4·709	=1·345	
Magnesia, with carbonic acid, .	0·023	=0·0092	
Magnesia, with other acids, .	0·8955	=0·3582	
Potash, ...	0·359	=0·0598	
Soda, ...	2·168	=0·5420	
Carbonic acid, with lime, .	0·554	=0·2015	
Carbonic acid, with magnesia,	0·024	=0·0087	
	21·3295		
Carbonic acid, atmospheric, .	1·676	=0·606	

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* Recalculated according to a more correct principle by Walker.

† Handwörterbuch der Chemie, Art. 'Analyse der Mineralwasser.'

Acids.		Bases.		Combined.
Sulphuric,	1·7146	Lime,	1·547	Carb. of iron, 0·0211
Chlorine,	·6080	Magne.	·3674	
Silica,	·213	Potash,	·0598	
Carbonic,	·2102	Soda,	·5420	
	2·7458		2·5162	
Silica deducted,	·213			
	2·5328			

Carbonic acid additional, (atmospheric) 0·606.

The silicic acid appears to be uncombined.

Though sulphate of lime still appears in large proportion, namely, 80·05 grms. in one imperial gallon, it is in smaller amount than in any of the former analyses, and even is below that of Walker, which is only 81·6 grms.

An important fact, however, deserves to be noticed, in reference to this subject. It is stated that the amount of carbonic acid varies very much; and that, though the average is 4·3 per cent., yet it may amount to 7·4, and 8·2, and even so high as 11·5 parts per cent. It appears, therefore, that the carbonic acid may not only be double, but nearly three times the amount of the average. It may be regarded as certain, that all this amount of carbonic acid has the effect of holding the lime in solution, forming a supercarbonate of lime. It may consequently be regarded as highly probable, that a large proportion of the sulphate of lime presented in the analysis, is the result of reaction, and the imperfection of the analytic processes. The carbonic acid being expelled, the lime is partly precipitated in the form of carbonate, partly forms sulphate of lime, by union with the sulphuric acid. If this reasoning be correct, then it may be inferred, that the proportion of sulphate of soda presented by the analysis should be increased, while that of the sulphate of lime should be diminished.

It seems reasonable to think, that at some future period, when the means of analysis are more perfect, and subject to fewer chances of error, the proportion of sulphate of lime may be diminished to a degree yet more considerable.

On the amount of nitrogen it is unnecessary to make any remarks. But for some observations on the presence of this gas in mineral waters, and its supposed effects on the human body, readers may consult what is stated in *Medical Intelligence,* in reference to the Buxton Mineral Waters.

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *A Treatise on Syphilis.* By HERBERT MAYO, F.R.S., Senior Surgeon of Middlesex Hospital, formerly one of the Professors of Anatomy and Surgery to the Royal College of Surgeons. London, 1840. (14th Feb.) 8vo, pp. 128.

2. *A Practical Treatise on the Venereal Disease, founded on Six Lectures on that subject, delivered in the Session* 1838–39, *at the Aldergate School of Medicine.* With Plates. By F. C. SKEY, F.R.S., Consulting Surgeon to the Charter-House, Assistant Surgeon St Bartholomew's Hospital, and Lecturer on the Principles and Practice of Surgery at the Aldersgate School of Medicine, &c., &c. London, 1840. (November.) 12mo, pp. 195.

3. *The Modern Treatment of Syphilitic Diseases, both Primary and Secondary, comprising numerous Formulæ for the Preparation and Mode of Administration of the New Remedies; and an account of a safe and successful method of treating Chronic, Protracted, and Constitutional Syphilis by the Mercurial Vapour-Bath.* By LANGSTON PARKER, Surgeon to the Queen's Hospital, Birmingham, &c. Second Edition. London, 1845. Post 8vo, pp. 228.

4. *The Treatment of Secondary, Constitutional, and Confirmed Syphilis, by a safe and successful method, with Numerous Cases and Clinical Observations, illustrating its Efficacy. and Mode of Application in the more Obstinate and Complicated Forms of the Disease.* By LANGSTON PARKER, Surgeon to the Queen's Hospital, &c. London, 1851. Post 8vo, pp. 112.

5. *A Practical Treatise on the Diseases of the Urinary and Generative Organs (in both Sexes).* Part I., *Non-Specific*

Diseases. Part II., *Syphilis.* By WILLIAM ACTON, late Surgeon to the Islington Dispensary, and formerly Externe at the Female Venereal Hospital, Paris. Second Edition. London, 1851. 8vo, pp. 693.

6. *A New Wayof treating Gonorrhœa.* By JOHN L. MILTON, M.R.C.S. London, 1852. 8vo, pp. 103.

Mr Acton distinguishes the Diseases of the Urinary and Generative Organs in both sexes into two great orders,—the first, Non-specific diseases; and the second, Syphilis. Under the former order are arranged such diseases as Gonorrhœa or *Blennorrhagia*, Balanitis or Inflammation of the Glans, and such affections as Warts or Vegetations, *Herpes præputialis*, *Ekzema*, and Excoriations. Under the second order are considered Syphilis in all its forms, primary, secondary, tertiary, and constitutional, Imaginary Syphilis, and Syphilitic Diseases in Infants.

He employs the term Venereal Diseases to include all those affections which, directly or indirectly, are the consequence of sexual intercourse. We understand from this that the term is either applied to include those arising from intercourse, whether pure or impure, or that it is understood that all sexual intercourse is impure.

Little doubt can be entertained of the great frequency and extensive prevalence of this disease in large and densely peopled cities and towns, and in maritime towns. As the number of professed public females is always considerable in these situations, and as many clandestinely lead lives incontinent and unchaste, it is impossible to doubt that of those who live in this manner, many are often affected by venereal diseases of different forms, and not a few are affected more than once, perhaps several times in the course of life. In the year 1716 Mr William Beckett, a learned surgeon at Abington, in Berkshire, speaks as if this class of diseases was most prevalent, not only in London, but in such places as Abington, Oxford, and Birmingham; and one great object of his writing an Essay,* somewhat strange, on the subject, was to place under public inspection and government all females who lived lives unchaste, whether in a public and avowed, or more private and concealed manner. This proceeding he argued was necessary and expedient on various accounts, but chiefly because these women almost constantly became diseased, and as certainly propagated and diffused disease of different forms among all those with whom they had intercourse, and hence through all the ramifications of society. He calculates that at least two thousand females of this character were requisite for London alone.

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* The Natural Secret History of both Sexes; or a Modest Defence of the Public Stews; with an Account of the Present State of Whoring in these Kingdoms. By Luke Ogle. The Fourth Edition. London, 1740. Mr Beckett, who here assumes the name of Luke Ogle, died at Abington on the 25th November 1738, in the fifty-fourth year of his age, and lies interred in St Helen's Church in that city.

With much coarse humour, and satirical jesting and irony, it is impossible to deny that there is in his observations and arguments a certain proportion of truth, mixed with a good deal of sophistry. It is often difficult to discover whether he is serious. Throughout he is rather the Democritus than Heraclitus, and every now and then his propositions and suggestions evidently belong to the ludicrous. On the other hand, all contemporary evidence, from the writings of Turner, Martin, Profily, and others, shew two things,—first, that sexual debauchery was extensive, almost universal, and under no restraint; and, secondly, that there was an infinite prevalence of diseases arising from illicit and irregular intercourse diffused through all the ramifications of society.

In other European countries this evil was rather greater if possible. In such countries as France, Italy, and Spain especially, the diseases arising from indiscriminate sexual intercourse seem, if we can believe the statements made by their physicians and medical writers, to have been so widely and extensively diffused, that to be free from disease was the exception, and to be under its operation was the rule. Don Antonio Nunez Ribeiro Sanchez, an eminent Spanish physician, who flourished during the first half of the eighteenth century, and published at Paris, in 1752, a learned and original Treatise on the Origin of Syphilis,* expresses the belief, in another treatise, which was published after his death by Andry,† that all the youth of every city in Europe, between the ages of sixteen and two-and-twenty, were tainted by some form of venereal disorder; in short, that the greatest part, he infers, of the youth, not excepting those of noble rank, are infected by this disease at the age of twenty-four; that is, that their systems have been thoroughly impregnated by the poison, and that they are then suffering under one or other chronic form of the disease.

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* Dissertation sur l'Origine de la Maladie Venerienne, pour prouver que le Mal n'est pas venu d'Amerique, mais qu'il a commencé en Europe par une Epidemie. A Paris, 1752. 12mo. Pp. 110.

† Observations sur les Maladies Veneriennes par feu M. Antoine Nunes Ribeiro Sanches. Publiées par M. Andry. A Paris, 1785. 12mo. Pp. 204, p. 21.

All these young persons, he adds, may be superficially cured; and, as the manifest symptoms have disappeared, they believe themselves radically cured. This belief may continue during the vigour of life, while all the functions are performing their duties well; but, when old age comes on, the chronic diseases, the effects of syphilitic poisoning, evince their presence, in proportion as years advance, according to temperament, and as individuals are robust or feeble.

Sanchez then draws a picture, sufficiently strong and sufficiently frightful, of the appearance of persons labouring under these maladies. But as these are the ordinary diseases of the decline of strength and vigour, the precursors or attendants of advancing life, it is unnecessary to reproduce the representations given by this learned observer. In truth, to believe Sanchez, every disease by which the human body is afflicted proceeds from, and is the result of, this infection by syphilis in early life. It can scarcely be doubted, that a disease which so readily and certainly affects the body locally and generally, must impair the health, and render the individual so infected liable to many other disorders. But it would require much more precise and pointed evidence than Sanchez has adduced, to convince the understanding, that the syphilitic poison is the cause of quartan agues, gout, rheumatism, hemiplegia, mania, deafness, jaundice, diseases of the heart, pains of the stomach, diabetes, and colic pains.

There were then in operation, and are in some places still in operation, other agents, which cannot fail to lay the foundation, both of various forms of bad health, and of premature old age, whether the syphilitic poison had been in operation or not. The general, the universal use of mercury to salivation, profuse and long continued, the intemperate and debauched lives which men led, the daily habit of drinking to excess spirituous liquors and spirituous wines, were alone sufficient to lay the foundation of many serious diseases, whether the individuals had or had not syphilis, added to the list. Sensual indulgences and intemperate habits, are always sufficient to induce diseases; and in this instance they operated in two modes. They not only led to the circumstances that caused the syphilitic infection, but, in all instances, they gave increased energy to the effects of this poison upon the frames of mankind, and they formed powerful obstacles to the means of effecting cures.

Let us not, however, in endeavouring to reduce to natural and reasonable dimensions, the large and hideous painting of the Spanish physician, be misunderstood. Far be it from our desire to do injustice to one who has shewn originality of mind in no common degree. In the dark, and perhaps overcharged painting of the extensive diffusion of the syphilitic poison in the first half of the eighteenth century, and especially in ascribing to this cause many diseases, and forms of disease, which faithful and unprejudiced observation shews to arise where the syphilitic poison is not in operation, and which take place whether it is in operation or not, Sanchez merely followed, or at least concurred in, views which were then common to physicians in almost all European countries. It is very likely that it aggravates, and had aggravated, all those unmanageable forms of disease, all those indications of the fragility of the human frame, which physicians and surgeons classify under the comprehensive and convenient generalization of Struma and Scrofulous distempers. We know as a fact, that the effects of syphilitic and other aphroditic disorders are always most severe and most unmanageable in those persons who present the marks of what are called the strumous diathesis. The question consequently is one which it is rendered difficult to decide in a positive manner; and it is not wonderful that Sanchez took the extreme view, and, like not a few continental physicians, up to the present time, ascribed to the operation of syphilis and aphroditic infection, a very great proportion of the most formidable diseases incident to the human body. It was an error in generalization easily made, and, considering all the circumstances, not easily avoided.

In his earliest Dissertation, published, if we mistake not, in his own lifetime, he evinces not only greater originality, and views more uncommon, but he maintains, with great learning and skill, the proposition which he undertakes to prove. At a time at which almost all, except our own countryman William Beckett, espoused, and more or less firmly believed, the doctrine originally taught by Leonard Schmaus, the historian Oviedo de Valdez, afterwards adopted by Astruc and our own historian William Robertson, and a multitude of other authorities, that syphilis was a distemper imported from America and the Antilles, Sanchez had the acuteness to perceive that the proofs of this statement are entirely loose and unfounded; and he forthwith undertook to prove, and proved in a manner which has never yet been properly refuted, two propositions; the first, that the disease was not, and could not have been, imported from America, if we trust to historical documents alone; and the second, that the distemper was, as had been thought by Leonicenus and Fracastorius, an epidemic malady, which began in Italy, and was rapidly disseminated and propagated almost at the same time in France and other European countries. In short, the disorder appears to have sprung up, as it were, in so many large towns almost at the same time, at Naples, Rome, Madrid, Paris, and even in the remote and then isolated town of Edinburgh, that the idea of successive propagation in the mode of a purely contagious disease, from one person to another, is, if not entirely excluded, at least rendered extremely improbable.*

From this digression, however, we return.

We have few means of proving with precision whether the prevalence of diseases, the consequence of sexual impurity, be as great as it was in the time of Sanchez and his contemporaries, or greater; or whether it be diminished. If we believe the representations of various writers, these disorders are not in France in any degree diminished. There is, perhaps, less malpractice by means of mercury; and fewer of the destructive effects which used to be seen when the use of that mineral was in its most palmy state, are now witnessed than formerly. But there are many diseases of females which are said, we know not with what truth, to be the result of impure connection, and the application of discharges from persons labouring under syphilitic and aphroditic diseases.

All observations derived from practice in civil life are necessarily imperfect and fallacious. In the army alone can it be expected that we can find correct information founded upon statistical returns. It appears that among a body of men of the aggregate force of 44,611, in the course of seven years and one quarter, there were 8072 instances of affections denominated venereal. Of these 8072, the number of 2449, or considerably more than one-fourth, were instances of *blennorrhagia*, probably more than once in the same individual. The number of 2144, that is, still more than one-fourth, was that of primary ulcers, not syphilitic. One thousand four hundred and fifteen (1415) were instances of primary syphilis, that is unequivocal chancre.

All the rest were either secondary syphilis, buboes, or affections of the testes, the urethra, or prepuce. Cachexia syphilitica, which, in a large proportion of cases, is rather *Cachexia mercurialis*, figures at the low rate of four cases in the eight thousand, that is, about one in two thousand; a result which it would probably be idle to ascribe to any other cause than the little use of mercury now made in the army. The total result is 181 cases of *aphroditic* disease in every thousand men per annum; a considerable number, when it is remembered that this is equivalent to saying that of a regiment six hundred strong, one hundred and eight men are annually affected by some form of aphroditic distemper, and consequently for the time unable to perform their duties as able-bodied soldiers.

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* Dissertation sur l'Origine de la Maladie Venerienne pour prouver, &c. A Paris 1752, Sections iv., v., vi., and vij.

Discipline is either much better, or moral restraint much more powerful in the army of the United States, and in the small force of Belgium. In the northern division of the United States army, among an aggregate body of 22,246 men, 971 instances of blennorrhagia occurred, and 462 instances of syphilis. This is one in twenty of urethral discharge, and one in forty-eight of syphilitic sore. In the southern division of the army of the United States, among an aggregate force of 24,979 men, 929 had blennorrhagia, or one in twenty-seven persons, and 584 had syphilis, or one in forty-three persons. Does this comparatively moderate prevalence of Aphroditic diseases depend on the continence of the men, the greater cleanliness of the females, or upon chance and accident? Do the soldiers of the United States army contain any of the leaven of their old English puritan progenitors? These are points which seem to demand explanation.

In the Belgian army, in which very strict precautions are taken to prevent the introduction of Aphroditic disorders, these complaints are said to be of rare occurrence. According to a statement by M. Vlemminck, inspector-general of health in the Belgian army, there are not more than one hundred and thirty persons affected by these complaints in the whole Belgian army, which consists of an effective force of from twenty-five to thirty thousand men. This is about one in two thousand soldiers. It thus appears that the Belgians, in the article either of continence or of good fortune, exceed even the Americans seven-fold at least.

The seamen of the British Navy are a little better in this respect than the soldiers of the British Army. Among an average aggregate force of 21,493 men employed in the home service during a period of seven years, that is, about the ports of the Nore, Portsmouth, and Plymouth, or on the coasts, it was found that 2880 persons laboured under Aphroditic disorders, or one in every seven men. Gonorrhœa and Syphilis are met with in nearly equal proportions in the Navy; one man in every seventeen being affected by the former disorder, and one man in every fifteen by the latter.

The effect of this prevalence of the disease in the navy must be considerable; as every man so diseased is rendered unfit for duty for at least one month.

In the merchant service, the disease is greatly more prevalent than either in the army or the navy. In the Dreadnought hospital ship, which it appears is devoted to the reception of this class of diseases, during a period of five years, there were admitted 13,081 patients, or upon an average 2616 patients annually. Of these 18,081 patients, 3703, or 740 patients annually, were suffering under different forms of Aphroditic disorders. These patients remain in hospital from twenty to twenty-four days, or upon an average twenty-two days and one-half; and the cost of their treatment for the four years has been £4165, 17s. 6d.

This greater degree of prevalence in the merchant service is evidently owing to two circumstances,—the relaxed discipline, or the want of proper control over the men, and the vessels, if mere carrying vessels, being in almost no instance provided with a surgeon. The effect of these is, that, in the first place, the men, who have more frequent opportunities of coming in contact with females, especially the low and unclean inhabitants of sea-port towns, get much more frequently and in larger numbers diseased; and when diseased, they are not compelled, as in the case of the navy, to make their ailments known to a surgeon, who might take prompt measures to stop the progress of the disease. It is a truth well known to practical surgeons, that an immense amount of disease prevails in such towns as Newcastle, Sunderland, Hull, Portsmouth, Plymouth, Southampton, Bristol, and Liverpool, though there are no means of getting at the exact numbers affected.

In London, it appears that the admissions for this class of diseases into St Bartholomew's Hospital are very large. Among 5327 out-patients admitted during one year under two of the surgeons, 2513, or nearly one-half, were suffering under different forms of venereal disease. Of this large number, 1995 were men, and 518 were women and children.

From these several facts, Mr Acton expresses, in strong terms, his doubt whether the disease was ever more prevalent at any former period than it is at the present time.

Mr Acton finds it necessary to controvert another opinion current regarding this class of diseases. It is supposed by many, and generally said, that the disease was, when it originally appeared, a very violent malady, and that in the course of three centuries it has lost much of its original severity, and is now, in the nineteenth century, a disorder comparatively mild and tractable. Mr Acton, on the contrary, thinks that the accounts in the original writers prove that it differs in no degree from what it is at the present day. He quotes the authority of Nicholas Poll and Oviedo, to shew that the natives of St Domingo,—the island from which it has been said the disorder sprung,—cured themselves in about ten days by means of guaiacum, though the Spaniards took between fifty and sixty days of treatment. Leo Africanus also, he adds, states that of the natives of Numidia he had seen many get well without either physic or physician, merely under the salubrious influence of the air.

> " Far then from thinking that syphilis has become milder, I only believe that public health is more studied, the treatment of the complaint better understood, and patients apply earlier, but the germ of the disease lurks amongst us, and in as concentrated a form as ever. This supposed mildness has ever been a favourite theory with authors who have written on the complaint, even immediately after the supposed origin of the disease; and its subsidence has been often prophesied: but the oracle has been false, and we are apparently as far from the fulfilment as ever."—(P. 11.)

It must be observed, that the author here assigns reasons why the disease is either more easily managed, or does not assume such formidable symptoms as it at one period did; but he does not exactly prove that it truly is, under the same circumstances, equally prevalent and equally destructive as during the beginning of the sixteenth century. It is also proper to allow, that Mr Acton seems to be quite aware of the influence of the circumstances of modes of life, intemperance, uncleanliness, and general inattention upon the characters as to severity of the distemper, and its power of injuring and destroying parts which it attacks.—(P. 10.)

We confess, that the more we have looked into the early history of this disease, the more are we inclined to think, that it really was at its first appearance in 1493, or rather in December 1494, and in February 1495, and for many years afterwards, a very violent, formidable, and unmanageable distemper. All the physicians and surgeons who then witnessed the disease, agree in describing it as a malady, which rapidly and irresistibly affected various parts of the body, producing on the skin and the mucous membranes nearest to the skin, the most loathsome, foul, eating, and intractable ulcers, rendering the patients at once offensive to others, and burdensome to themselves. Such, in a few words, may be said to be the representations of the multiplied authorities, quoted and referred to by such writers as Astruc and Sanchez. So great was the consternation of physicians and surgeons at witnessing its ravages, that they in a manner avowed their inability to cope with the disorder, and left patients to take very much their own way, in attempting to relieve and cure themselves.

The symptoms which gave most trouble, and excited most apprehension in the minds of the early surgeons, were the deep-eating ulcerations, the pains in the bones, connected with tuberosities and hard tumours or skirrosities, as they are called by Joannes de Vigo, and the eventual destruction of the bones by caries. So early, however, as the year one thousand four hundred and ninety-seven, only three years after the first appearance of the distemper, physicians and surgeons had begun to employ quicksilver, though only externally and locally, as means of healing the sores and eruptions, because these have some analogy to leprosy, for the cure of which the mineral had been so employed. Under this local treatment it appears not a few good cures had been effected; and John de Vigo, besides Vidius, and other surgeons, practised this method, as has been fully explained by the former. Vidius is stated to have employed fumigations; which Fracastorius was disposed to condemn. The treatment hitherto appears to have been confined to the local application of mercury; for it does not clearly appear, that when fumigations are recommended, they contained mercury. But there is little doubt that, though the local application, either in dressing sores or anointing affected parts, was intended to cleanse ulcers, destroy fungous matter, and to produce incarnation, yet by this means, the system was often brought under the influence of the mineral. Vigo particularly recommends that the skin should be anointed with an ointment containing quicksilver, until the patient complains that the teeth are slightly set on edge.

In the mean time, Jacopo Berengario of Carpi, known as one of the earliest anatomists of Italy, professor of anatomy, first at Pavia, afterwards at Bologna, and eminent as a surgeon, bethought himself of attempting to treat the disease by means of mercury, in the form of ointment rubbed over the body.* At first, his success with this mineral was very great; and from the immense numbers of persons who either were cured or seemed to be cured, Berengarius was regarded as the most skilful surgeon in Italy, at that time the seat of all science, literature, and art. By the adoption of this method of inunction, Berengarius, it was said, realised immense wealth in a short time. The cures, however proved, in not a few instances, to be very temporary, and the patients returned in a worse condition than that in which they formerly were. The clamour raised against him in 1524, if we can believe Benvenuto Cellini, who was one of his patients, was so violent and outrageous, that if they could have gotten him, they would have torn him in pieces. Cellini, who in his dissipated and vagabond life had caught the disease, had been treated by mercury, first with benefit, but afterwards, according to his own account, with great and lasting injury; and he has left a strong testimony against the mercurial treatment, which he represents to have interrupted him in the prosecution of his profession, and caused great loss and inconvenience.*

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* Fallopius, De Morb. Gallico, c. lxxvi., p. 728.

It has been occasionally doubted, whether Joannes de Vigo or Jacopo Berengario first employed quicksilver in the treatment of *Lues Venerea.* Vigo seems to have employed the mineral at first locally, and gives, in his PRACTICA COPIOSA IN ARTE CHIRURGICA, which was commenced in the year one thousand five hundred and three, and concluded in one thousand five hundred and thirteen, full directions for the preparation of mercurial ointments, plasters, and cerates. But Berengarius was probably the first who practised the systematic use of mercurial unction and frictions over various parts of the cutaneous surface. The botanist, Peter Andrew Matthiolus, is said to have been the first who administered quicksilver internally.

The main point, however, to which at present it is requisite to direct attention is, that quicksilver by no means proved a perfect and permanent remedy for the evils of *Lues.* Multitudes of persons in Italy and Germany either remained uncured, notwithstanding its use, or seemed by its employment to be rendered worse. Such at least is the testimony of Ulrich de Hutten, as well as Cellini. They then had recourse to guaiacum, and afterwards zarza parilla. The therapeutic merits of the former, which had been tried in many hundred instances before 1517, were loudly and strongly proclaimed by Ulrich de Hutten in the year 1519; and the well-known character of this man, who was a friend of Erasmus and of Luther, and one of the great agitators in the cause of the Reformation, tended to introduce the remedy into general use. The credit, in short, of quicksilver, was much impaired; and various persons, as Fernel and others, denounced it as a most pernicious and hurtful agent.

From the testimonies of various contemporary authors, two facts, apparently at variance, seem to be clearly established. The first is, that the disease was, in many instances, in certain individuals, and in certain circumstances and seasons, quite as severe, quite as destructive as, and not less incurable than, at its first appearance. The same accounts of loathsome eating ulcers, of foul eruptions or affections of the throat, nose, and eyes, destruction of the bones of the face and skull, nodes and caries of the bones generally,—occur from time to time, and numerous are the instances of deaths imputed to the disease. The second is this,—that within thirty-five years from its first appearance, in 1494, the disease had three times changed its appearance and its characters. For the first twenty years, that is till 1514, it retained its original violence and severity, destroying by ulceration the generative organs, producing pustular and tubercular eruptions on the face and scalp, destroying the uvula and palate, the nasal and ethmoid bones, the lacrymal bone, and causing bad ozænas and *fistula lacrymalis.* After this period it became in not a few instances milder. The pustular eruptions and the eating ulcers of the skin, probably *ekthyma,* were greatly less frequent; even the bones are said not to have been so often affected; though nodes and periosteal swellings still appeared, and caused much pain to patients, and great trouble to surgeons. It appears, in short, that in the time of Fallopius, who was born in 1523, and died in 1563, that the disease was in many respects much changed as to severity, greatly milder, and rarely presented the same characters of destructiveness which it possessed when it first appeared in 1494.* This fact seems to be established by the testimony of so many credible authorities, that without strong and particular evidence to the contrary, it seems not reasonable to call it in question.

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* Life of Benvenuto Cellini, written by himself, and translated from the Italian by Nugent; also, Vita di Benvenuto Cellini Orifice et Scultore Florentini, &c. Firenze, 1829. Tre volume. Volume ii., p. 116.

The two facts now mentioned, contrary as they appear at first sight to be, may probably admit of explanation, if to offer explanation of such a matter appear to be worth while. The explanation seems to be found in the physical circumstances in which patients were placed, and in the treatment to which they were subjected, especially in the early stage of the disease.

The first circumstance which undoubtedly aggravated the disease at its earliest appearance, was that of uncleanliness, general and personal. The personal filthiness in which almost all persons then lived, was alone sufficient to produce disease, and certainly greatly aggravated all the diseases incident to the human body. Soap was almost unknown, and its use was confined to very small numbers. No baths, little or no washing; both the surface of the body was left covered with the filth of years, and articles of clothing were allowed to remain impregnated and saturated with the animal secretions for years. In addition to this, the houses and the apartments in which people in those days lived, were not only so constructed that they could not be kept clean, but they formed receptacles of all sorts of unclean things. The description which Erasmus gives of English houses in the sixteenth century was equally applicable to the houses of France, Germany, and Italy, excepting some of the magnificent Italian palaces. The habits of the clergy also, especially some of the regulars and monastic orders, were not favourable to cleanliness, which many of these persons regarded as a luxury or sensual gratification too sinful for a good Christian to enjoy.

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* Gabrielis Fallopii De Morbo Gallico Tractatus Patavii, 1594, apud Luisini Collectionem, cap. xxiii.

Intemperance in the use of food and drink, with the general use of salted, pickled, food during a great part of the year, unquestionably contributed to aggravate all the symptoms of *Lues Venerea.* Fresh animal food was accessible only for a few months in summer. At all other times the articles used were salted, pickled, smoked, not unfrequently putrid. Vegetables were scarcely known, and were used only as luxuries at the meals of the great and the wealthy. The potato had not been introduced. Gardening was little known, and the art of rearing vegetables, such as carrots, parsnips, coleworts, pease, was scarcely known in some countries. Large quantities of strong beer, strong spirituous wines and spirituous liquors were constantly drank, and were regarded as necessaries of life by those who could procure them.

According to Ulrich de Hutten, the disease, soon after its commencement, made great progress in Germany, and was there more extensively diffused than in any other place. This, says De Hutten, I ascribe to our greater intemperance than that of other nations.* It is not a matter of doubt, that the intemperance here mentioned, refers at once to large eating and to copious drinking.

Exposure to all the vicissitudes of weather must also be regarded as a powerful aggravating cause. Every fact concurs to shew, that, while persons of both sexes were indifferent to exposure out of doors and upon journeys, their accommodation within houses was such that it excluded neither cold nor moisture.

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* De Morbo Gallico. A Treatise of the French Disease, published above 200 years past, by Mr Ulrich de Hutten, Knight of Almayn in Germany. Translated into English by a Canon of Marten-Abbye. Now again Revised and Recommended, &c. By Daniel Turner, of the College of Physicians in London. London, 1730. 8vo, pp. 110. See p. 3.

When to all these circumstances is added the liberal and repeated administration of quicksilver, it cannot be matter of wonder that the disease assumed severe and aggravated characters. The actual facts of the mischief done by quicksilver are known from the testimony of Benvenuto Cellini, Fracastorius, Ulrich de Hutten, Fernel, and many other authorities; and as it is not a matter of doubt, that, while this mineral was exhibited, little or no pains were taken to regulate diet and protect patients from cold, instead of appearing singular that mischief was produced in all such circumstances, it would have been matter of wonder if all the symptoms were not aggravated by many degrees. Cellini, after he had taken much quicksilver by order of Berengarius without benefit, cured himself, he informs us, by the use of guaiacum.

Again, though exposure to the vicissitudes of weather was manifestly hurtful, not less injurious was the hot, suffocating treatment to which many patients were subjected. According to the account of De Hutten, and other contemporary writers, when they were subjected to anointings, as he calls them, the patients were shut up in a stove, with continual and fervent heat, some twenty, some thirty whole days. Some lying in beds within the stove, he says, were thus anointed and covered with many clothes, being compelled to sweat. Part at the second anointing began to faint. Yet with all this heating treatment hardly one such person in a hundred could be cured in this way, so that the cure held but for a few days.* He adds himself, that he underwent this torture eleven times in the course of nine years with great peril and jeopardy of life, and without any manner of benefit or relief.†

The question still remains, Why did the disease become in many instances less severe and less destructive in its ravages? This question it is not very easy to answer. Probably it really underwent, as other epidemic distempers have undergone, a change in its characters. But exclusive of this, which is mere supposition, patients and physicians became in the course of thirty years more cautious, more careful, more attentive to cleanliness and cleanlier modes of living, more attentive to diet, more temperate in the use of intoxicating drinks; and eschewing quicksilver, they trusted much to guaiacum, zarza, and sassafras. Ulrich de Hutten published in 1519 his Treatise on the French Disease, and in recommendation of guaiacum; and this, with the mishaps attending the practice of Berengario, the doubts of Fracastorius, and the decided opposition of Fernel, contributed greatly to impair confidence in the use of quicksilver. The use of this mineral accordingly was either limited, conducted upon more careful principles, or altogether abandoned. All these circumstances in all probability tended to mitigate the violence of the disease.

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* De Morbo Gallico. English Translation of Turner, p. 9.

† *Ibid.*, p. 10.

The question, nevertheless, as to the influence, favourable or hurtful of quicksilver, will probably not be soon decided. At one time it seemed pretty clearly established, that to the operation of quicksilver were owing many of the most formidable and unmanageable symptoms which were at one time ascribed to Lues itself. All the severe symptoms of the bones, especially the superficial bones, the skull, the nasal bones, the os unguis, and the ozæna, as well as the nodes, and bad ulcers on the shins, *ulnae*, and similar parts, seemed to be distinctly attributable to the action of mercury; and all practical observers have met with not a few instances which prove this inference. Others again deny all this, and maintain that these effects are merely indications that quicksilver has not been given to sufficient amount, and that the disease is still not cured. Into this field of disputation it is not our intention at present to enter. But we must be permitted to say, that, notwithstanding the high authorities of several surgeons for the use of this mineral, even in the form of the liquor of Van Swieten, the results of observation afford little encouragement to its administration.

Purulent discharges from the urethra are of two kinds. Either they are simple and spontaneous; or they are virulent, and dependent on the introduction of an animal poison. To these discharges the general name of *Gonorrhœa* has been given by most nosologists. This name, however, was founded upon a gross error as to the nature and source of the discharge. It was manifestly not seminal fluid that was discharged, but purulent matter from the mucous membrane and perhaps the lacunæ of the urethra. Aware of this error, Frederick Swediaur, in 1785, proposed to discard entirely the term *Gonorrhœa*, and substitute in its stead that of BLENNORRHAGIA, from Βλεννος, *mucus*, and ῥηγνυμι, *rumpo*, that is, *mucifluxus activus*. He also proposed to employ the term BLENNORRHŒA to designate Gleet, or the uninflammatory discharge.* This proposition was either neglected or received no attention for the space of fully forty years, perhaps longer. It was then taken up and adopted as their own by Ricord and other writers; and by their example it has been introduced into the nomenclature of this part of medicine. This term Mr Acton adopts provisionally, as it were, because he thinks the time is not yet arrived when a surgeon may tell his patient that it is not gonorrhœa, but *blennorrhagia* under which he is suffering. Mr Parker puts it down as a synonym, and adheres to the old term for common use.

One century and thirty-seven years have elapsed since it was first said, and three-fourths of a century have passed since it was proved, that BLENNORRHAGIA, or the virulent puriform discharge from the urethra may have no connection with Syphilis. It is, indeed, clear that this sort of discharge was known to medical practitioners long before the time assigned for the appearance of *Lues Venerea;* and if evidence were wanted upon this head, it must be allowed that the facts supplied by William Beckett of Abington were quite satisfactory. It was long, nevertheless, supposed to be the result of the introduction of the syphilitic poison into the urethra, until it was maintained by Zeller and Cockburn,† that it was an affection arising spontaneously, and not necessarily connected with, the syphilitic poison. The same proposition was afterwards strongly maintained by Sigwart,‡ Benjamin Bell, Andrew Duncan senior, and John Clement Tode;§ and it has since that time been a general opinion that urethral puriform inflammation is the effect, not of the syphilitic virus, but of a poison altogether distinct.

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* "The name *Gonorrhœa* is derived from the Greek *γονη, genitura, semen*, and *ῥεω, fluo, i. e., fluxus seminis;* which is a very improper name for the disorder in question, because it conveys an erroneous idea. The name *Gonorrhœa* implies a discharge of semen, which never takes place in this disorder. If a Greek name is to be retained, I would call it *Blennorrhagia*, from *βλεννος, mucus*, and *ῥεω, fluo, i. e., Mucifluxus* (*activus*); and thus distinguish it both from real *Gonorrhœas*, and from *Gleets*, to which latter I would give the name *Blenorrhœa, Mucifluxus* (*passivus*), *i. e.*, without phlogistic symptoms. In English, the disease is commonly called a *Clap*, from the old French word *clapier*, a single lodging of a common woman; in German, a *Tripper*, from dripping; and in French, a *Chaude-pisse*, from the heat and scalding in making water; names derived from the principal symptoms of the disease."—(*Practical Observations on Venereal Complaints*. By F. Swediaur, M.D. 2d Edit. London, 1786. 8vo.)

Swediaur commits a small mistake in deriving the latter component of *Blennorrhagia* from *ῥεω*. Its second component part is *ῥηγνυμι, rumpo*.

† The Symptoms, Nature, Cause, and Cure of a Gonorrhœa. By William Cockburn, M.D. London. 1715. 8vo.

‡ Dissertatio de Gonorrhœa Virulenta sine Contagio. Tubingae, 1764.

§ Joh. Clement Tode, Erinnerungen für Aerzte und Kranke die den Tripper heilen wollen. Copenhagen, 1777. 8vo.

This opinion, indeed, was rather strongly controverted both by Swediaur, John Hunter, and Nisbet,[*] soon after it had been brought forward. The arguments of J. Hunter are well known. Swediaur especially concludes, that gonorrhœa frequently arises from the same venereal poison applied to the inside of the urethra, which, when applied to the glans, prepuce, &c., produces chancres, or, when absorbed into the system, produce *lues;* and that there are gonorrhœas which owe their origin either to acrimonious substances introduced into the urethra, or perhaps sometimes to a more violent mechanical stimulus arising during coition, or from some other causes hitherto unknown. Notwithstanding the weight of these and similar authorities, however, the opinion of the distinctness of the Blennorrhagic and Syphilitic poison has been gradually and steadily gaining ground, and may now be regarded as established.

Of this opinion an elaborate defence was given in 1812 by Hernandez. *Blennorrhagia,* in truth, is not Syphilis. The only person almost by whom, of late years, this doctrine has been maintained, is the late Dr Wallace of Dublin.

This question is to a certain extent connected with another, whether there be simple or spontaneous gonorrhœa, and another variety, the virulent or poisonous. Both Mr Acton and Mr Parke give their adhesion to the doctrine which is now regarded as the common one, that BLENNORRHAGIA is of an origin and nature altogether distinct and different from Syphilis. So also the late Mr Mayo decidedly espouses the same side, and removes one of the objections which have been long urged against the doctrine. It is well known that it has been repeatedly said, that gonorrhœal matter applied to the denuded surface of the glans and prepuce, is followed by the formation of sores which eventually lead to constitutional symptoms. This was completely contradicted by the result of the experiments of M. Ricord, which, though amounting to upwards of three hundred, never gave any but negative results. Mr Mayo had several times inoculated with their own discharge persons labouring under gonorrhœa; and like M. Ricord, he observed no pustule to follow. In one patient, however, who was labouring at the same under gonorrhœa and chancre of the prepuce, he performed the double experiment of inoculation from both sources. Mr Mayo had

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[†] First Lines of the Theory and Practice in Venereal Diseases. By William Nisbet, M.D., &c. Edinburgh, 1787. Pp. 65–69.

expected that the one puncture would produce no result, and the other a chancre. Equal and similar sores followed both punctures. Mr Mayo thinks, that the explanation is the following. Immediately within the orifice of the urethra was a small chancre, which could be distinctly seen on pressing the lips of the urethra apart. Mr Mayo doubts not, that from this source, the matter of the urethra derived the property which it possessed of producing a chancre; and he thinks it just to infer, that a similar explanation is to be given of the case by John Hunter, and all similar instances.—(Pp. 39, 40.)

It is not so easy to understand what are the exact views of Mr Skey on this point. After allowing that Lues Venerea may produce a purulent discharge from the urethra, Gonorrhoea, he informs the reader, may be the product of a poison similar to itself, introduced into the urethra during sexual intercourse, or may arise from other causes, independent of the presence of gonorrhœal matter, in the party from whom it is contracted. Mr Skey, in short, admits the existence of a simple and spontaneous gonorrhœa, and of a venereal gonorrhœa. Yet he afterwards allows, that he hardly knows what to understand by the term Venereal Gonorrhœa. "If it be acute discharge," says Mr Skey, "communicated by venereal (sexual) intercourse, who will venture to assert, that another acute form arising spontaneously in one sex, when promoted by rheumatism, a circumstance of daily occurrence, may not convey its poison during intercourse to the other, to be possibly communicated hereafter to any number of persons subject to its influence."—(P. 171.)

Mr Skey had immediately before stated the interval of time between exposure and the appearance of the discharge, as affording a good diagnostic mark between simple and spontaneous Blennorrhagia on the one hand, and that which is virulent and poisonous on the other. Here, however, he sets aside this criterion, and declares, that all the other symptoms being the same, he cannot entertain a doubt, that a very considerable proportion of cases of gonorrhœa, are not the product of a specific poison.

This, it is impossible to doubt, although Mr Skey expresses the opinion, that the authority of the old doctrines, that is those, we understand, subsequent to the date of Mr Hunter's work, 1788, has so prejudiced the reasoning of these writers, that the spontaneous origin of gonorrhœa is never admitted. To us it appears, that the prejudice lies in the opposite direction. It is well known, that long previous to the appearance of the work of John Hunter, the spontaneous origin and the independence of blennorrhagia or puriform discharge from the urethra upon the syphilitic virus, was admitted; and there is no doubt that even previous to the alleged date of the appearance of *Lues Venerea*, these puriform discharges were known to surgeons.

The arguments which appear to weigh with Mr Skey for the spontaneous origin of gonorrhœa, are its occasional origin from the matter of leucorrhœa, and from connection with a female about the time of menstruation, and its origin in females after mechanical injury. In all these instances he maintains the discharge contains no specific poison, yet it communicates a gonorrhœa to the male.

> "But it has been argued," he says, "by those who distrust those views, why do not these diseases frequently attack married men? Why should either sex be exempt from them? I believe that they are not exempt; that such diseases occur not unfrequently, in the early period of married life. At least I am satisfied, that our sex by no means rarely pays the penalty of seduction, more especially of very young women, and even when the act is not completed. I can readily conceive reasons why it is not more frequent in married life; but on them I shall not now dwell. It is sufficient for my purpose, that gonorrhœa does occasionally occur between healthy persons having intercourse with none others."—(P. 174.)

A good deal of this is probably a figurative mode of speaking. The truth is, that strictly speaking, all that is here implied relates to the difficulty of distinguishing between virulent gonorrhœa, and that which is produced in some other mode. Puriform discharges from the urethra, not to be distinguished in physical characters and physiological effects, arise after connection with females labouring under leucorrhœa, at the period of menstruation, and probably during states in which the utero-vaginal membrane is not perfectly healthy. Are these to be regarded as examples of virulent and poisonous gonorrhœa? This is the question for consideration.

Mr Acton seems, by his mode of viewing the discharge, to think that the most virulent and well-marked cases may take place under every variety of physical and physiological circumstances. Thus, according to his view, not only age, sex, temperament, climate, and mode of dressing and living, and frequent sexual intercourse, may cause blennorrhagia; but local irritations, as bougies, pessaries, calculi, enemata, injections, the conditions left after labour or abortion, menstruation, and above all, inattention to cleanliness, may be followed by attacks of blennorrhagia. Little doubt can be entertained, that these agents may produce mucous or puriform discharges. Whether they can produce the genuine blennorrhagic secretion, will remain, we think, an open if not a disputed question.

It is right, however, that Mr Acton should be heard on one of the least equivocal of the physical conditions now adverted to, that of neglect of cleanliness and INATTENTION TO CLEANLINESS.

> "I have been accused of traducing, in a former edition, the character of my countrywomen. Considerable experience in private practice has, however, shewn that such has not been the case; and it is incredible how inattentive women, even those living in the greatest splendour, are to the necessary ablution of these parts. I have been told that my observations apply only to prostitutes, and that class of women. Surgeons, however, who are consulted on uterine affections, and on venereal diseases, have reason to know that the women of the town are particularly careful, and employ plenty of water; and it would be the height of prudery for a surgeon, whose object it is to enlighten his professional brethren on uterine disease, to conceal, from motives of false delicacy, facts which are too apparent, and which can be verified by those who have the means or opportunities of doing so. But if instrumental examinations are never made, how can any one assert that I am wrong in my conclusions? I might here mention many cases; one will suffice. An old patient of mine married, and shortly before his wife's confinement came to me with gonorrhœa præputialis. He assured me that since his marriage he had led a most exemplary life. On examination of the lady (and she moved in the highest circles), I found an acrid discharge, more than enough to account for her husband's condition, and which was completely removed by tepid water, which she had been afraid of using. Surely, accoucheurs would do well to give their patients a few hints on the necessity of ablution, even up to the period of their confinement, which might be done without in any respect wounding their feelings.
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> "*Menstruation* has its influence in producing the affection in question. Of this fact no people were more aware than the Jews. We find it strictly forbidden in the Mosaic law to have connection with a woman about this period, and the command, no doubt, arose out of the fact that such intercourse was found to produce blennorrhagic affections. In the present day this cause is frequently urged as the one which has produced the complaint, and, I have little doubt, with considerable truth; but instrumental examination too often proves that there is another and more potent-cause, namely, ulceration of the neck of the uterus, which, under circumstances to be mentioned hereafter, produces too often discharges in the male."—(Pp. 35, 36.)

It cannot be doubted, that inattention to cleanliness, especially personal, often causes disease; and we have admitted and shewn the influence of this circumstance at the first appearance of Lues Venerea in European countries. If it does not favour the production of morbid poisons in the human body, and in the bodies of animals, it certainly gives increased force and violence to all the effects of these poisons. It must be allowed, nevertheless, that it is not proved, that the circumstances enumerated by Mr Acton have produced genuine virulent gonorrhœa or *blennorrhagia*. They have produced discharges closely resembling this discharge, perhaps not to be distinguished from it by our present means of diagnosis. If these circumstances, either separately or conjointly, have produced blennorrhagia, then it follows that this foul and contaminating secretion has been produced and generated millions of times, and that it is daily and hourly produced and extinguished. We do not see, if the facts and arguments of Mr Skey and Mr Acton be admitted to have force and weight, how it is possible to escape from this conclusion. It is true, that to counteract and neutralize this inference, Mr Acton says, that thus considered, that is, in reference to predisposing and exciting causes, " blennorrhagia presents nothing that can be called specific." " It may arise under the most varied circumstances and causes; its existence in the male or female, therefore, is itself no proof of libertinism; it may occur in the most modest female, as well as in the youngest child. In Medical Jurisprudence, the necessity of being guarded in opinion is obvious; and the surgeon in family disputes on the subject of contagion, should be especially cautious, and always lean to the weak side."

The plain interpretation of this, if it mean anything beyond a figure of speech, is either that the difficulty in forming a diagnosis from present symptoms is so great, that it is impossible to give a decided opinion as to the exact nature of any given puriform or purulent discharge from the urethra of the male and the vagina of the female; or that though the surgeon can do so, he is either to suppress the truth, or take no means to ascertain the truth. Every practical man has met with instances of puriform discharges in persons who have declared most positively, and with every appearance of truth, that they have had no communication with any female except their own wives; even in young girls, who either were not exposed, or could not have been exposed, to the matter of what is called virulent blennorrhagia, these puriform discharges from the vagina have been observed. Such cases as these will, we believe, occur at all times, and in all states of society. But to us it appears that these are not produced by the true blennorrhagic poison, if such a poison is to be allowed to exist, or the poison must be generated *de novo* from time to time.

If we allow that the diagnosis is difficult, probably under certain circumstances impracticable, then this is another mode of solving the difficulty; and from what we have seen and learned, we believe that it will eventually resolve itself into something like this. The following cases from Mr Acton may shew here what is meant.

Difficulty of Diagnosis.

"As the knowledge of cases that have actually happened may put surgeons on their guard, and enable them to avoid similar errors of judgment, I will relate the following, told me by an eminent practitioner in London.—A lady's-maid consulted him for some complaint, and after a few days referred it to the lower part of the abdomen. He persuaded her to allow an examination, when (according to his statement) the hymen was found entire. There was, however, considerable discharge per vaginam, with pain in making water, and purulent secretion from the urethra. He told her he suspected the disease being venereal; she denied it, went back and related what had passed to her mistress, whose husband came to my friend; spoke of the young woman's good previous character; asked if he believed that the complaint was venereal, if so, threatened to turn her off; and thus placed the surgeon in no very enviable position. The result was, that my friend explained to the master the difficulty of diagnosis; the servant kept her place, and the surgeon acknowledges having lost the confidence of the family, who believe him ignorant of this part of his profession. I have never been placed in a similar position, but if I were, much as I should wish to screen the female, I should certainly let her know that the surgeon has a character to save as well as she has, and quickly remind her that her *breach of confidence* has obliged the surgeon to take measures to support his opinion. However, I think a little tact may always prevent these unpleasant occurrences. Another case, illustrative of the power a medical man possesses of amicably arranging family feuds, is shewn in the following instance, and is the type of others that are daily occurring.

"A very respectable-looking female applied to me for a discharge of twenty months' standing, which she asserted her husband had given her. Instrumental examination detected a large ulceration of the neck of the uterus. This patient stated, that twenty months before she had miscarried, and the discharge had been increasing ever since; and as she had observed stains on her husband's linen, she was sure he had gone astray, and that she had contracted the foul disease from him. He denied the accusation, and accused her of infidelity, and they led a most unhappy life. In about three weeks after, this female was cured, the husband soon got well, and they are now perfectly satisfied that the affection in the one was the consequence of the miscarriage, and the clap in the other a consequence of the previous affection in the female.

"The reader who has attentively perused the foregoing pages must feel convinced that blennorrhagia does not necessarily depend upon a specific virus, but is, as I have stated in the commencement of this chapter, a non-virulent, not specific disease."—(Pp. 37, 38.)

After this not very satisfactory discussion, Mr Acton, still satisfied that *Blennorrhagia* may take place independent of contagion, proceeds to consider whether it may be the result of this principle, in other words, of a specific and peculiar poison. The whole doctrine, however, may be said to be destroyed by the following admission. "When in the following pages the word contagion is employed, let it be understood to mean, that if the muco-purulent secretion produced by any of the foregoing causes of *blennorrhagia*, comes in contact with another portion of previously healthy mucous membrane either in the same individual or in another, it will in many cases, but not necessarily in all, produce a similar affection; not, however, by virtue of anything specific in the muco-purulent secretion, but merely as a simple chemical irritant."

He then enters into a sort of discussion, in which it is not easy to perceive that anything fixed is concluded. Surgeons must not be too credulous; that is, they must not believe that females have not been going astray, when they protest, in the most positive manner, that they are innocent. Women often invent stories as to how they have got contaminated. These stories must not be believed. This, then, is the counterpart of leaning to the weak side formerly recommended. Here the surgeon is not to lean to the weak side, but to incline to the opposite direction. Males, often gentlemen, get good veritable discharges, as like *blennorrhagia* as possible. They protest they have no connection with any but their wives, or perhaps some quiet female of most unsuspected morals, living in a decent locality, and, it may be, respectably known. The female, also, married or not, is not to be questioned; she is virtue herself. What is to be done? Mr Acton tells you, that, to all protestations of virtue and innocence, he thinks not of questioning the fidelity of the ladies or the gentlemen. That is a matter to be settled at home. It is simply his duty to examine and report on the state of the health of the party, that is, the nature of the discharge, whether true or spurious blennorrhagia.

> "The number of examinations I have thus made are very numerous, and I almost always find a constant train of symptoms. The female is a delicate, fair, pale creature, who has been suffering in her general health for years, liable to indigestion, nervousness, and its attendant evils, complaining of pain in the back, shooting down the front of the thighs, able to undergo little exertion, and in consequence leading a most indolent life; accustomed to confined bowels, irregular menstruation, amounting sometimes to amenorrhœa, in other instances attended with the most profuse hæmorrhage, complaining of more or

less pain in sexual intercourse, and admitting or denying the existence of *whites* or *weakness*, as females call it, in the short intervals between menstruation.

"Instrumental examination detects a lax pale vagina, with more or less profuse glairy discharge, frequently with simple ulceration of the anterior or posterior lip of the uterus, the opening of which may be completely blocked up with the glairy white-of-egg discharge; entangled in this may be noticed globules of pus, and the secretion from the vagina may be more or less purulent, but on pressing the urethra no pus passes out. In some cases the uterus may be enlarged, in others displaced, from the laxity of the parts; the examination is attended with very little pain, and in cases in which the patient suffers, it appears to depend upon nervousness more than upon any other cause.

"Now, what answer can you give your patient, and what does all you have seen teach you?

"An examination thus made enables you to tell your patient that the female from whom he has contracted the disease has been long labouring under the disease called 'Whites;' that it is this complaint which has produced the discharge in him, and your examination has given you no reason to believe that she has been unfaithful; in fact, as a general rule, the sexual desire in such females has been reduced to the lowest ebb, and infidelity is not one of their sins; but by degrees they have got into such a state that local treatment will alone cure the complaint, and treatment of indigestion will avail nothing."—(Pp. 40, 41.)

This is merely stating, in more circuitous terms, what has been long known to many observers, and what, it has been already said, Mr Skey advances.

It further appears that the communication by contagion depends greatly upon the state of the parts from which the secretion proceeds. So long as the secretions of the female are void of purulent matter, and the utero-vaginal membrane and follicles are in the chronic state, giving rise to white opake discharges, not purulent, and when sexual intercourse is rare and not frequent, contamination, that is, affection of the male urethra, Mr Acton thinks will not probably take place. If, on the other hand, sexual intercourse be frequent, and be accompanied with much excitement on the part of the male, then inflammation is established in the utero-vaginal membrane, the secretion becomes purulent, and contagion results. This, he adds, is still more likely to take place, if the male is in the habit of indulging in malt liquor and strong drink, and lives very freely.

In reference to this subject, Mr Skey has an idea, which deserves notice. It would be observed, that in speaking of gonorrhœa, he talks of its occurrence and development as being promoted by rheumatism. The idea of Mr Skey, so far as we understand, is, that in persons labouring under rheumatism, either an urethral discharge is liable to take place spontaneously, or it is liable to be easily induced by intercourse with a female, though in perfect health, or at least not affected by *blennorrhagia*. In short, when rheumatism and gonorrhœa take place in the same individual, it is not the gonorrhœa that causes the rheumatism, but the rheumatism that causes the gonorrhœa. It is well known, that gout often induces a puriform discharge from the urethra; and it is not impossible that rheumatism may do the same. But there are two difficulties in this case of coincidence. We have often seen them together; but never, that we can remember, did we see the rheumatism precede the gonorrhœa; always it either followed it, or accompanied it very closely. *Secondly*, the persons who are the subjects of this conjoined form of distemper had, in all cases, done as much as lay in their power to contract both distempers. Excess in eating and drinking, hours late and early, exposure to night air, to chills, to wettings, generally when in a state that could not be called sober, not unfrequently a slumber for an hour or so on the ground, on the grass, or some equally exposed place, were the usual antecedents of the two distempers. The discharge shews itself first; but in no long time, that is the usual time for an attack to come on after thorough exposure to cold, the rheumatic symptoms follow.

It would lead to unseasonable digression to follow this subject in all its ramifications. The idea of Mr Skey evidently resolves itself into this, that an inflammatory and disordered state of the system predisposes to *blennorrhagia*. The opinions of Mr Acton will come under observation immediately.

It appears, therefore, that in certain cases, and probably that of the man Connor, who mortally stabbed a female from whom he contracted *blennorrhagia* (see page 43) is one of them,—the development of this discharge is as much to be ascribed to the condition of the system of the male sinner as the female.

There is yet another antecedent which ought not to be omitted. This is the promiscuous intercourse so uniform among persons of loose and immoral habits. In circumstances in which the same female admits several males, and the converse, it is manifest that there must be great irritation of the parts, that attention to cleanliness is out of the question, that this irritation alone may be sufficient to give to the secretions poisonous and contaminating properties. This, however, is what Mr Acton, apparently in imitation of M. Ricord, calls epidemic blennorrhagia, and of which he adduces an example from what was observed not long ago in Paris, by the circumstance of a great number of workmen collected in one spot, and living in common with a few women suffering under *blennorrhagia*. It is manifestly the epidemic of uncleanness, frequent irritation of the parts, and most debauched habits. All those people, it may be observed, eat and drink strong liquor to excess.

On the question of the interval which usually elapses between exposure and the appearance of the discharge, Mr Acton refers only to one case, rather doubtful, in which it appeared next morning, that is after a lapse of about twelve hours or a little more. He treats with doubt the instance given by Benjamin Bell, in which it was said to have appeared on the fiftieth day. The determination of the point is involved in difficulties, which are too obvious to require to be specified.

On the subject of METASTASIS, which embraces that of Gonorrhœal Rheumatism, the most eligible course is to place before our readers the observations of the author.

> "Patients will tell you, that on the last occasion Mr So-and-so *would* not cure the clap speedily, for fear of driving it into the system, and having been recommended to you, they hope you will pot effect a rapid cure at the expense of their general health. In the present day, these prejudices have still to be counteracted; not that I believe the profession at large entertain them, but as they lurk in old-fashioned corners, and every now and then appear, a few words must be said on the subject.
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> "I fear in many instances these ideas must be traced to the inability practitioners experience in curing rapidly discharges from the male and female organs of generation. I have heard of surgeons who find it advisable to propagate these opinions, which are thought to point out the danger of applying to modern practitioners, and cause certain old-womanish treatment to be voted safe, and reconcile the patient to submit to a nine months' treatment of drugs and lotions, and, as I said before, are constantly used to veil ignorance. There are others, however, who appear conscientiously convinced of the truth of these dogmas, and would not, if they could, cure their patients. Whether the doctor or the patient is most to be pitied, it is difficult to say, but my own experience may be of use to the one and the other, and may, I hope, be the means of completely routing out this old-fashioned doctrine.
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> "In the first place, it is untrue to assert that a discharge rapidly cured will be followed by any general disease. The modern practice of rapidly curing gonorrhœa sets this question entirely at rest. As to the cases of rheumatism (and I shall have much to say hereafter on this subject), I may assert that, in nine cases out of ten, the rheu-

matic affection will not be caused by repressing a clap, inasmuch as it is generally found impossible to cure the discharge (when once freely established) in cases where rheumatism co-exists. I have at the present moment under my care, a gentleman, who no sooner contracts a discharge, than he immediately applies to me in order that I may instantly check it by the most active remedies; experience has taught him that unless the discharge from the urethra is instantly cured, the complaint will inevitably be followed by rheumatism of a most severe form. In the commencement of my London practice, I entirely disagreed with these practical views of patients, but more extended experience has made me reflect upon these obscure cases, and I am obliged to confess that in many constitutions the only way to avoid rheumatism coming on, is to rapidly cure the discharge, the portal apparently through which rheumatism enters the constitution. I will detail a case which has been seen by many besides myself in London, and, unfortunately, without affording him any relief, until after months of treatment.

" A gentleman, still under twenty-six, was subject as a boy to rheumatic gout (so he says); a few years ago he contracted his first clap, which was attended with a most severe form of rheumatism, in spite of all that London surgeons and physicians could do for him; and a residence on the Continent was the only means by which a cure was effected. Having been such a martyr to the complaint, and resolved not to run the chances of infection again from promiscuous intercourse, he married; some few months after his nuptials I treated him for a severe attack of rheumatism which followed the influenza; he recovered rapidly from these complaints, and remained perfectly well; his wife, however, began to complain of leucorrhœa, and my patient came again under my care with the slightest possible discharge. Knowing all the circumstances of the case, I advised some simple astringent wash and aperient medicine; instead of abating, the discharge increased, and was followed, in spite of all advice, and the best concerted measures, by rheumatic ophthalmia, inflammation of the bladder, general rheumatism; and the disease, in a sub-acute form, was no sooner cured in one set of muscles, than it broke out in another. Perhaps it may be said thut I drove the disease into the system; unfortunately for such doctrines, I was for some months unable to master the discharge, it went on uncontrolled by all remedies, a stumbling-block to the best medical and surgical opinions in London."—(Pp. 46–48.)

The pathological state of the urethral canal in *blennorrhagia* has been examined by several observers, Morgagni, Maximilian Stoll, John Hunter, and Sir Astley Cooper. The author gives the account of the last observer, as the one which he thinks most just.

" Many years ago I had an opportunity of examining the urethra of a man who was executed, and who had gonorrhœa at the time of his execution. The inflammation had extended down to the bulb of the urethra, for an inch to an inch and a half down; the urethra was exceedingly red, and there was some effusion of matter on the internal surface; the urethra was red at the bulb, but not of so deep a colour. The inflammation is therefore not confined to an inch or an inch and a half down the urethra, but often extends over the bulb of the urethra, and in this way often produces stricture. In the case to which I allude, the gonorrhœal inflammation had extended at least seven inches down the urethra. In general, on examination of a subject who has died under gonorrhœa, you will find a small quantity of purulent matter at the extremity of the penis, and inflammation extending about an inch and a half down the urethra, which, if exposed to the air for twenty-four hours, assumes a blood redness.

The general summary is, that the mucous membrane, whether of the male urethra, or the female vagnia, is reddened, swelled, and lax; sometimes a little firm. In some instances in the female, the vaginal membrane presents erosions, while the epithelium is reddened. In other instances, the epithelium is detached or abraded. The membrane is bloodshot, and discharges blood from slight causes. Mr Acton states, that in the chronic state of the disease, he has observed vegetations in the whole course of the vagina, as well as at the orifice of the urethra. Ulcers may also be situate in the vagina, generally about one inch and a half above the carunculæ myrtiformes.

These facts shew the morbid anatomy of the disease in its ordinary forms. There are, however, severe forms in which the inflammation, from neglect, intemperance, and mismanagement on the part of the patient, or oversight in the surgeon, is no longer a superficial spreading inflammation, as it naturally is, but passes to the submucous tissue, and produces in the male great mischief and formidable symptoms. The urethra becomes contracted from the thickening and swelling of the submucous tissue; the discharge of urine is partially or entirely obstructed; there is pain along the course of the urethra; and sometimes the penis itself is swelled. The patient shivers and is feverish, and in truth, labours under inflammation of the urethra. In other instances, the disease spreads backward to the membranous part of the urethra, the prostate gland, and the neck of the bladder. The same contraction and partial obliteration of the canal takes place; and the patient who is feverish and shivers, suffers greatly until relief is obtained.

The most effectual remedy in this case is bloodletting, and the warm bath, with the use of aperient medicine. In some instances it is good to apply leeches along the perinæum.

Mr Acton notices complication with abscesses, ulcers in the vagina, hæmorrhage, and chancres, all of which are quite familiar to surgeons.

On the subject of diagnosis, nothing precise is communicated. Indeed, all attempts to give distinguishing marks between mild, simple, or spontaneous blennorrhagia, and what is called virulent, and to which the property of contamination has been ascribed, have failed. In speaking of the diagnosis between mild blennorrhagia and the virulent disease, he adduces the test furnished by M. Ricord ; namely, that by inoculation. This has been already mentioned in speaking of the observations of the late Dr Mayo ; this is its dependence upon or complication with chancre. This, Mr Acton calls the *certain pathognomonic diagnosis* of a virulent blennorrhagia. The truth is, it is a complication depending upon a disease totally different. It may be left to readers to judge whether this is or is not diagnosis.

Treatment, Mr Acton considers under three heads ; *first*, the prophylactic or preventive treatment ; *secondly*, the ektrotic or abortive treatment, or that the object of which is to cut short the disease before it can be completely established ; and *thirdly*, the curative treatment, or the consideration of those means and remedies which have the effect of stopping the secretion when established without injury to the parts. Under the first head, Mr Acton is found reviving the proposition made in 1716 by Mr William Becket, in recommending the establishment of a board of inspection to control, superintend and cure public females when labouring under disease. As, however, this is not likely to meet with favour, he recommends, that surgeons should exhort this large body of females to observe cleanliness, and employ astringent lotions and cooling washes. This is enough to shew how likely the advice is to be listened to and obeyed. Conceive the ladies of Wapping, Saffron Hill, and St Giles,' or those of the High Street and Canongate Closes in Edinburgh, recommended to keep themselves clean, and wash regularly morning and evening. Would it not be possible to advise the former to abandon drinking gin, and the latter whisky, and both classes to refrain from picking and stealing ? We believe that this class of females, at once wicked and unfortunate, suffer great privations and great evils in the course of their calling. But it is not by the medical man only entreating them to observe personal cleanliness, that their condition is to be amended ; many things must precede that. The answer indeed to the proposition, is, that they never look near a medical man until they are so ill that they cannot go about at all. The only and most effectual prophylactic treatment, is for the male sex to avoid females of this character and all courses of the kind, and above all, to abstain from the use of spirits in all forms, and all drinking to excess. It is by the encouragement which these women receive, that the body is kept up and replenished by frequent recruits.

Ektrotic treatment is to be attempted by the two orders of means, of general agents and local applications. It is unnecessary to repeat the details.

The Curative treatment consists, in like manner, in the use of general remedies and local applications. Under the former head are specified general blood-letting, the use of leeches with the caution not to apply them on depending parts, we should say actually inflamed parts, warm baths, the use of diluent drinks, and attention to the state of the diet and the digestive organs.

To the latter belong injections, which, however, are to be used cautiously.

Mr Milton makes the treatment a much more serious business. He informs his readers that nothing is more unsettled and unsatisfactory than the treatment of gonorrhœa; and having before his eyes the fear of stricture and other evils, he is on the one hand desirous to impress the necessity of curing the disease promptly, but, on the other hand, he is anxious to accomplish this object without leaving behind it those pernicious and painful consequences which too frequently ensue on this disease. To this we may hereafter advert.

Meanwhile we proceed to the subject of Blennorrhagia in the male, which is treated in the second chapter of the work of Mr Acton.

This appears under two forms, BALANITIS or inflammation of the glans, and *Blennorrhagia Urethræ*, or what may be called *Urethrorrhœa*.

The term BLENNORRHAGIA BALANI was first introduced by Swediaur, whom we have already mentioned as an onomatopoeist, to designate *blennorrhagia*, or puriform inflammation of the *glans penis*. BALANOS (Βαλανος) is the Greek for *glans*, a nut or acorn; and hence its derivative was chosen to designate that sort of inflammation especially which is liable to attack the glans in *blennorrhagia*. From Swediaur, after being neglected for fifty years,* it has been adopted in the abridged form of BALANITIS by Ricord, Rayer, and other French writers, and from them received into use by various English authors, who were not in all circumstances aware of the original source. The affection thus designated consists in redness and swelling of the glans and inside of the prepuce, often with irregularly shaped patches of erosion and ulceration, and generally a puriform or mucopurulent discharge. This discharge is generally secreted in abundance, so that each time when the glans is examined, it is found covered by the secretion; and if it be wiped off, it is replaced in a very short time by a fresh exudation. The epithelial or cutaneo-mucous covering of the glans, is red, raw, and moist, like that of a blistered surface.

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* The First Edition of the work of Dr Swediaur was published in London in 1784. A reprint of this was published in 1786. A Third Edition, with some Corrections and Additions, was published in 1788. A reprint, at London, was called a fourth edition. In 1798, there appeared at Paris an edition, which is in many respects a new and original work.

This complaint may produce for so limited a part great evils. It causes phimosis and paraphimosis, indeed is a most common cause of these affections; it gives rise to abscesses of the prepuce and frenum, sometimes to direct fistulous perforations of the former; and the whole prepuce has been known to slough off from the violence of the inflammation. More rarely, the glans itself is affected by mortification.

This disorder is often complicated with chancre, that is an ulcer of syphilitic origin.

Exclusive of this last circumstance, however, *Balanitis* does not give rise to secondary or constitutional symptoms. It may cause the formation of a bubo in the groin; but the glandular swelling is the result of simple inflammatory irritation, and which used to be called sympathetic bubo, and is not owing to the absorption of poisonous matter.

The treatment of *balanitis* in uncomplicated cases is simple. Ablution is of much moment. Next to this, the application of dry lint, if practicable between the prepuce and glans, tends greatly to dry up the discharge and allay the inflammation. The lint should be removed and renewed twice daily. Washes and ointments are in general not useful, sometimes hurtful. If the parts continue inflamed or eroded after this, the best treatment is the application of the solid nitrate of silver drawn gently over the parts.

Under this head is given a digression about administering quicksilver in cases in which there are sores and genuine syphilitic ulcers on the penis. Some of the London surgeons, it seems, imagine that M. Ricord exhibits the mineral in these circumstances; and against this doctrine Mr Acton protests with not small indignation, for ascribing to M. Ricord such a practice. On the contrary, he says, that experiment has thousands of times (?) proved that the disease, that is, the ulcers, are specific in character, yet can be cured, and are, under our treatment, entirely removed in the majority of cases, without mercury. The digression is altogether out of place, and seems to have dropped into the present position (p. 74) by some of those strange accidents which happen to authors. But whether seasonable or not, surely we did not need to go either to Paris or to M. Ricord to learn, that syphilitic sores can be perfectly well cured without the use of quicksilver, and that the administration of that mineral has caused infinitely more mischief than all other methods of treatment taken together.

In the second section is treated Urethral *Blennorrhagia*, or what we have ventured to call *Urethria* and *Urethorrhœa*.

The disease is so well known, that it is totally superfluous to dwell on it.

The method of treatment most eligible is the great point.

Mr Milton is disposed to deride the opinion of John Hunter, that in the great majority of cases of gonorrhœa, medicine has little or no control on the disease. Others have maintained, that their methods and applications have certain control over it, and will assuredly cure it. Mr Acton is for stopping the discharge at once, and leaving nothing to time, to diet, or the powers of nature. Mr Milton has entered into an elaborate and painstaking, and, we have no doubt, a very faithful examination of the comparative merits of all the best methods that have been in more or less use during the last thirty-five or forty years. These methods are,—1st, by copaiba, twelve cases; 2d, by means of aperients, twenty-nine cases; 3d, by purgatives, seven cases; 4th, with strong injections of nitrate of silver, eighteen cases; 5th, with injections of chloride of zinc, twenty-two cases; 6th, with the caustic plug,—a piece of calico rolled into a cylinder, and saturated with a strong solution of nitrate of silver,—four cases.

Besides these tabular lists, he considers the effects of such remedies as turpentine, cubebs; antiphlogistic measures; diuretics,—as spirit of nitrous ether, nitre, acetate of potass, *liquor potassae;* alteratives;—restrained diet, diluent drinks; and, in short, all means that have been at any time, or by any person, tried.

The following are the conclusions at which Mr Milton arrives, after all this survey of therapeutic methods.

"1. That the remedies enumerated, though adequate to cure by far the greater number of cases, yet leave many unrelieved.

"2. That some of them are disagreeable, some dangerous, and some futle.

"3. That there are no rules laid down to guide us in distinguishing at the outset those cases which are, from those which are not, amenable to these remedies, the divisions given by Eisenmann, Swediaur, &c., having no bearing on this point.

"4. That where so large a list of remedies is given, some attempt ought to be made to decide exactly in what cases certain of them should be tried, which has not yet been done, thus making the cases obtained but a blind chance, only calculated to mislead, and forming no basis for future operations.

"5. That the reputation of injections has been injured by no certain rules being established as to the relative value of the different substances recommended, or the strength in which they ought to be employed in different individuals; thus leading to the indiscriminate application of many different substances of different strength to the same case, or the equally indiscriminate application of injections of the same strength to cases not equally fitted to bear them.

"6. That treatment has been made secondary to disputes about the nature, sources, and history, of this disease; whereas the cure ought to precede all other considerations; for, however great the value of science, the welfare of man is a still greater object to every well-regulated mind."—(Pp. 33, 34.)

Perhaps the reader, after these conclusions, will wonder what surgeons have been doing for the last three hundred years. Some of the conclusions are reasonable enough. Others apply more to the misapplication of means than to the means themselves. Is gonorrhœa really a disease so unmanageable? We can, from experience, assert, that it is not so, providing proper measures be taken in good season. Scarcely is there any disease so easily or so certainly cured, if proper and decided means be adopted, and if the patient be made to adhere to the use of these means. The main reason of the apparent and alleged difficulty of curing cases of gonorrhœa, arises, on the one hand, from want of decision and firmness on the part of surgeons, and from the irregular, desultory, and debauched conduct of patients. These persons are, almost all, with few exceptions, sensual, debauched creatures, who never think of self-denial, who refuse themselves no gratification, whatever be the cost and danger attending it, and to whom the slightest restraint in diet, and withdrawing their usual allowance of malt liquor, spirits, and wine, are things altogether intolerable. Under the observance of restricted diet, for from ten to fourteen days, rest, abstinence from all stimuli, with an occasional dose of aperient medicine, most cases, even of very bad gonorrhœa, will get well. If more decided measures are required, they may be found in the use of the antimonial solution, or tincture of digitalis, or ipecacuan wine. But unless the patient give up the use of animal food, in every form, and refrain from every article except water, or the watery ptisans, all is unavailing.

The great value of these measures is, that, under their employment, the disease gradually subsides, without leaving after it any of those changes in the mucous membrane of the urethra which commonly follow other methods, and without the necessity of having recourse to such agents as copaiba, cubebs, turpentine, or Venice turpentine, all of which cure the disease, only by establishing another morbid state. From a considerable number of instances, also, which we have treated, we have reason to believe that the use of injections, in the majority of cases treated in the manner now specified, is entirely superseded, and quite unnecessary.

These ideas, we perceive, will be palatable to neither Mr Acton nor to Mr Milton, who, like many other very worthy members of the profession, are for doing all by medicine and medical and surgical remedies. We have no desire to claim exclusive confidence for the method now recommended, or to ascribe to it more value than it is justly entitled to. We shall therefore allow Mr Acton still to employ injections, and Mr Milton to follow his example, which he does pretty closely.

The former is a decided advocate for the use of the nitrate of silver injection; but, to avoid mischief, he performs the injection himself, and the following form part of his directions after the solution has been thrown into the urethra.

> "I have now no fear of leaving my patient, having applied a suspensary bandage, or what answers equally well, a handkerchief passed round the loins, and another tied in front and behind to support the testes, with strict injunctions to abstain from any kind of fluid whatever, so that the urethra should enjoy a few hours repose. On the next morning the discharge has either altogether ceased, or a drop only is to be seen, and in the course of the day a mere weeping from the urethra occurs. The patient may now take fluids in moderation, consisting either of tea or soda-water, but coffee should be strictly prohibited, as well as wine, beer, and spirits. In some cases, towards the afternoon, the discharge returns again. If this becomes green or yellowish, I have no scruple in repeating the injection, with similar precautions; but I seldom have recourse to a third, and my patient is quit of a troublesome complaint in a very few days.
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> "The effect of nitrate of silver in a strong solution is very surprising, as may be judged of from the previous description. It appears to possess a specific action in changing the vitality of the mucous membrane, sustituting for the original inflammation a new one, whose principal characteristic is its short duration; it destroys, likewise, the morbid element.
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> "Out of the large number of cases that have been treated on this

plan, I have never yet seen any ill consequence arise, not even hæmorrhage or retention of urine. I may mention, however, that I have never employed the treatment except in private practice, where I have had the assurance that my patient would implicitly follow my directions. I should not recommend the treatment among out-patients at public institutions, nor, in fact, have any expectation that any abortive plan will generally succeed with them, but, on the contrary, be attended with such consequences as would soon prevent a surgeon from employing the treatment in private."—(P. 90.)

With this method, Mr Acton assures the reader he has had very great success; and the only difficulty in the way of its perfect success is, that, in young men, sometimes one injection of nitrate of silver does not cure the discharge. This he ascribes to them not coming in time.

Mr Acton, then, has a long discussion upon the therapeutic powers of copaiba, and the best mode of administering this nauseous tell-tale medicine, on the use of cubeb pepper, with an account of the peculiar method of M. Ricord. This statement contains tolerably satisfactory evidence of the effects of this method of treating *blennorrhagia*.

It is not unworthy of attention, that, amidst all the partiality which various authors evince for their own methods, and, above all, for the employment of irritating medicines which cure one disease only by inducing another, some glimmerings of sense and reflection occasionally break out, and speak to the reader a more useful lesson than all the discussions about the comparative merits of different methods of medication, and different forms of what have been sometimes called the confounding method. The following observations, by Mr Acton are, in this point of view, of considerable value; and, to some simple persons, it will be matter of wonder that they are made to conclude this section.

" If a rational plan of general and local treatment is followed, the symptoms subside in a few days; but there are irritable constitutions which give us great trouble. During the last year I have had two such cases; one occurred in a medical man. The sub-acute symptoms continued in spite of all my own and others' endeavours to stop them, and time alone seemed to benefit my patient, as he gradually got well; but I am induced to think that he was reduced too low at first, as the cure was effected under a tonic treatment carefully arranged. These irritable persons will hardly bear either a depleting or stimulating plan; they are a class by themselves. The inflammatory symptoms do not run high, but persist in a most annoying way. Bearing these hints in mind, the young surgeon should treat such patients very cautiously.

> "I have lately had a case under my care, shewing the difficulties a surgeon meets with in actual practice. A barrister who had been in a very delicate state of health for some months, contracted gonorrhœa. His stomach would not bear copaiba or cubebs; a state of depression when they were given came on that was unbearable; enemata containing these preparations were tried, but irritability of the rectum obliged me to leave them off; injections could not be borne by the urethra, and time and attention to diet alone succeeded in ultimately curing this patient."—(P. 112.)

If this be the case, and, we believe, that, if inquired into, it will be found much oftener to be so than is generally supposed, where can be the use of what Mr Acton denominates specifics, such as cubebs or copaiba.

The subjects of chronic gonorrhœa, stricture, and prostatic inflammation, with their treatment, follow. The author is at much pains to exhibit minute views of these complaints; does not always agree either with predecessors or contemporaries, except Ricord, who is his great authority; and certainly he has collected much information on the pathology and treatment required by the disease in its several forms and stages.

The following extract, on the treatment required for inflammation of the prostate gland, is all that we can at present make room for.

> "The canal passing through its substance is swollen, and puffy, and the spasm of the surrounding structures completely impedes the flow of urine. In these aggravated cases, reliance must not long be made upon the means above alluded to; the deep situation of the inflamed structures prevents our remedies acting as they do in other parts of the body, and the importance of drawing off the urine becomes of such moment, that I do not now hesitate in recommending the early use of instruments, following them up by an antiphlogistic and soothing plan. Here again, however, the surgeon meets with difficulties he is unprepared for in the early part of his practice; for he has spasm, a diminished canal, and an enlarged prostate, in addition to the pain felt by the passage of the instrument along the urethra; and it requires very careful and delicate manipulation of any instrument to cause it to enter the bladder without producing mischief. Sir B. Brodie recommends the elastic catheter without the stilet; this, in my private practice, has not succeeded, and I attribute the failure to an impossibility of tilting the point over the inflamed prostate. I have found no such difficulty with the silver catheter of a moderate size, taking care to draw the penis well forward, at the moment that the point of the instrument is progressing, and then depressing the handle to an extent that a novice is unprepared for. As soon as the water is drawn off, the patient's symptoms abate; the urine will be found high-coloured, often tinged with blood, acid, and loaded with mucous or pus globules. On withdrawing the instru-

ment—and there is no occasion to leave it in the bladder—a few drops of blood may follow, and a soreness along the course of the canal remains. In a short time the desire of relieving the bladder will occur, the scalding accompanying the flow of urine; still the stream will not be so small as may be imagined, particularly if energetic treatment has been in the mean time employed. Generally, however, recourse must be had to the silver catheter. Although a little water may pass, we may often suspect that the bladder is unrelieved; particularly when there is some pain above the pubes, and percussion on that region furnishes a dull sound, with desire to micturate frequently, and relief obtained only for a short time; the occasional passage of the instrument will not only relieve these symptoms, but prevent any future disease of the bladder."—(P. 186.)

Next follow the subjects of disease of Cowper's Glands, Epididymitis, Irritable Testis, Inflammation of the Bladder, Spermatorrhœa, and Impotency. All these subjects are treated in great detail.

In the Third Chapter, the subject of *Blennorrhagia* in the female is treated. In the Fourth Chapter, the author considers *Blennorrhagia* as common to both sexes. And in Chapter Fifth, he gives an account of the Etiology of Warts or Vegetations, *Herpes Præputialis*, *Ekzema*, and Excoriations. This concludes Part First, which occupies more than one-half of the volume.

Here, for the present, we are reminded that we must make a pause.

We have only a few words to add on the merits of the individual treatises enumerated at the beginning of this article.

The small treatise of the late Dr Herbert Mayo is, like all the productions of that well-informed and intelligent man, remarkable for its acuteness, the originality of its views, and the instructiveness of the observations. Though not large, it contains a great amount of practical information. Of Gonorrhœa, Dr Mayo says little, except incidentally. Of Syphilis, he says a great deal in a condensed form. Dr Mayo is quite aware of the hurtful effects of quicksilver upon the system, and accordingly dissuades from its employment. The treatise forms a good short manual on the subject.

The practical treatise of Mr Skey is truly what it professes to be, with perhaps something more. Mr Skey evidently derives his descriptions and deductions from much practical observation. Always instructive, often he is original, sometimes paradoxical in some of his views. He often differs from John Hunter and other authorities; but he always assigns for this difference good reasons. Mr Skey is an opponent of the administration of mercury, and is quite aware of its hurtful effects; but while he allows that the disease can be cured perfectly well without the use of quicksilver, he oddly arrives at the conclusion that its administration is essential. The treatise cannot fail to be read with interest and advantage.

Of the two works of Mr Parker, the small treatise on the Modern Treatment of Syphilis forms a very useful condensed view of Aphroditic diseases and their treatment. Mr Parker is a great advocate for the use of quicksilver; and the second treatise is intended to illustrate his method of employing this mineral, in such a way, he mentions, as to insure its beneficial, and exclude its hurtful effects. For details on this head the reader may consult the *Medical Intelligence.*

Of the work of Mr Acton we have already spoken at sufficient length, to render it unnecessary to add anything here. The work is elaborate, and contains full information on all the divisions of the subject. It would be improved, however, were it a little more condensed.

The small treatise by Mr Milton forms an excellent statistical record upon the Therapeutics of *Blennorrhagia.*

ART. II.—1. *A Practical Treatise on Diseases Affecting the Skin.* By the late ANTONY TODD THOMSON, M.D., F.L.S. Fellow of the Royal College of Physicians, Professor of Materia Medica, and of Medical Jurisprudence in University College, and Physician to University College Hospital. Completed and Edited by EDMOND A. PARKES, M.D., Member of the Royal College of Physicians, Professor of Clinical Medicine in University College, and Physician to University College Hospital. London, 1850. 8vo, pp. 440.

2. *Thomson's Conspectus of the British Pharmacopœias.* Sixteenth Edition. Edited by EDMOND LLOYD BIRKETT, M.D., Cantab., Fellow of the Royal College of Physicians, Physician to the City of London Hospital for Diseases of the Chest, Curator of Museum of Guy's Hospital. London, 1852. 12mo, pp. 216.

3. *The London Dispensatory: a Practical Synopsis of Materia Medica, Pharmacy, and Therapeutics; Illustrated with many useful Tables and Woodcuts of the Pharmaceutical Apparatus.* By the late ANTONY TODD THOMSON, M.D., F.L.S., Professor of Materia Medica, Therapeutics, and Medical Jurisprudence, in University College, London; Fellow of the Royal College of Physicians of London; Fellow of the Medical, the Speculative, and the Royal Physical Societies of Edinburgh, &c., &c. Eleventh Edition, Edited by ALFRED BARING GARROD, M.D., Licentiate of the Royal College of Physicians; Professor of Materia Medica and Therapeutics in University College, London; and Physician to University College Hospital, &c., &c. London, 1852. 8vo. Pp. 1280.

Antony Todd Thomson was born at Edinburgh on the seventh day of January, one thousand seven hundred and seventy-eight. His father, Mr Alexander Thomson, by birth a Scot, had settled in America, where he held two lucrative appointments under the British Government; being Postmaster-General of the Province of Georgia, and Collector of Customs for the town of Savannah. His mother was the daughter of an American gentleman named Spencer.

Some obscurity rests upon the parentage of the father of Mr Alexander Thomson. It is only known that he was born in the year 1741, and supposed, by family tradition and other evidences, to be connected with the family of Southesk, whose peerage is still under attainder. This Mr Thomson held a high office in the Excise Department for Scotland, the business of which was then conducted in Edinburgh.

Mr Alexander Thomson happened to be in Edinburgh on a visit to his native country, at the time when his son came into the world. The well-known Antony Todd was then Postmaster-General in England. He was the intimate friend of Mr Alexander Thomson, and performed the duty of godfather to the son. To the good offices of this gentleman, whose personal influence was confirmed by the marriage of his daughter with the Earl of Lauderdale, Dr Thomson afterwards owed his first step to independence.

The mother of Antony Todd Thomson died when he was one year old, leaving him in so feeble and precarious a state of health, that it was considered proper to consign him to the care of a relative Mrs Rainie, to whom his father was subsequently united in a second marriage. To this lady, to her care, her precepts, her example, Dr Antony Todd Thomson was accustomed to ascribe many of those habits of self-denial and principles of integrity, from which he never departed.

After a long course of oppressive and tyrannical conduct on the one side, and determined resistance upon the other, actual hostilities had broken out in America in the year 1775; and it was three years after this when Mr Alexander Thomson came to Scotland. Till the autumn of 1778, the war had been chiefly confined to the north-eastern provinces of New York, the Jerseys, Pennsylvania, and the borders of Connecticut, when the southern counties enjoyed profound tranquillity, excepting only some petty hostilities between the colonies of Georgia and New Florida. Now, however, the British commander determined to carry the war into Georgia. By one decisive attack upon Savannah, on the 23d December 1778, the whole province fell into the hands of the British, before the close of 1778. This success, however, was not lasting. The capitulation of Lord Cornwallis to General Washington, on the 17th of October 1781, decided the independence of the American Colonies, and enabled them to take decided measures as an independent Government.

It was probably about this time, 1781, that Mr Thomson returned to America, accompanied by some of his family. The son, Antony Todd Thomson, was at this time sufficiently old to retain a distinct remembrance of the voyage to the American continent, as well as what he witnessed on his arrival. He was accustomed to speak with interest of the appearance of a tribe of Indians at Savannah, where his father resided. As soon as the United States Government was established, all were called upon to avow their allegiance to the States, or declare their adherence to the mother country. Mr Thomson having refused to take the oaths to the United States Government, was summoned to New York. On the voyage thither, the whole party were captured by a French frigate, but were afterwards exchanged. Mr Thomson then resigned his appointments, amounting in value to more than two thousand pounds yearly, and returned with other American loyalists to England, to live, it is said, upon the hopes of compensation.

After many delays, a small pension was granted to him; and upon this he maintained and educated his family, living in the immediate neighbourhood of Edinburgh, in humble circumstances, but with the respectability always conferred by good character. This may have been in 1783 or 1784.

Mr Thomson had fixed his residence with his family at the

Sciennes, a suburb of Edinburgh, at the south-east corner of what is now called the Meadows. At Hope Park, in the immediate neighbourhood, lived Baron Cockburn, the father of Henry Cockburn, now one of the Judges of the Court of Session. This contiguity brought the young members of the family in communication, at a time when both boys were not more than six years old. In this manner commenced a friendship, which continued without interruption till the death of Antony Todd Thomson. During this time, it appears, he underwent the usual course of training at the High School.

The time came, however, when it was necessary to think of a profession. Mr Thomson senior, influenced by his American connections, had formed the scheme of sending both Antony Todd Thomson and his elder brother to America, in some department connected with architecture. This plan, however, though excellent, probably as the means of making a fortune, both sons frustrated, by having formed different views of their own.

Antony Todd Thomson had indeed at this time formed, probably about 1795, the design of studying medicine at the University; but this he concealed from his parents, from the consciousness that they were unable to provide the requisite funds. These were only to be obtained after some sacrifice of time and some self-denial. Through the interest of his godfather, Antony Todd, a clerkship in the Post-Office was obtained, at a salary, competent in these days, and gradually rising to seventy pounds. The office required little or no mental application, and occupied in these days little time. From this small seed, observes Lord Cockburn, his whole fortune grew. He instantly devoted himself to reading and the requisite studies, with as much zeal as his circumstances admitted. Having acquired the rudiments of medical knowledge, and his salary being raised to seventy pounds annually, —a sum equivalent, in all respects, to one hundred and forty or one hundred and fifty at present,—he ceased to require paternal aid, and commenced a course of regular and systematic study, by attending the lectures on the branches of medical science then taught in the College, with a view to qualify himself for the practice of medicine. The medical professors of whom he was wont to speak most highly, were Monro the second, Joseph Black, the professor of chemistry, and James Gregory, professor of the practice of medicine. He attended also the lectures of Dugald Stewart.

Antony Todd Thomson appears, indeed, at this time not to have confined his attention and study exclusively to medicine. He became, on the 27th of February 1798, a member of the

Speculative Society, a literary and debating association, consisting chiefly of students of law, in which were discussed subjects literary, metaphysical, and political. This Society, distinguished by the eminent men who have been its members, and most useful in cultivating the growing powers of its speakers, was at that time in full activity; and every Tuesday evening, its discussions were animated by Jeffrey, Francis Horner, Brougham, Lord Lansdowne, and several others, who have since rendered themselves extensively known. Mr Thomson spoke rarely. He read two Essays, one on Instinct, the other on Tragedy, and its utility to society.

In November 1799, he became a member of the Royal Medical Society; but the meetings of this he seems to have attended only one season. It is stated that, in the same year he had obtained his degree as Doctor in Medicine. This, however, must be a mistake. The name of Antony Todd Thomson does not appear in the List of Graduates at Edinburgh; and he had published the London Dispensatory in 1811 without the title of Doctor in Medicine. Soon after November 1799 he went to London.

Mr Thomson thought first of endeavouring to obtain an appointment in the East India Company's Service. But eventually he resolved to settle in England. His own wishes would have led him to commence professional practice as a physician; but in compliance, it is said, with the wishes of others, he began as general practitioner. He began business in Chelsea, aided, it is added, by the whole pecuniary resources, not large, of his two sisters. The loan was afterwards punctually repaid, and Mr Thomson was ever after anxious to shew to these relatives both his affection and his gratitude, by kind attention to their comforts in after life.

For a long time the success of Mr Thomson, like that of all beginners, was doubtful. But accident contributed to bring him into notice. One morning at an early hour he was walking, according to custom, in the Park; as he passed by the Serpentine River, the body of a man had just been taken out of the water. Life apparently was extinct; but Mr Thomson fortunately succeeded in restoring the man. The name of the medical practitioner was mentioned in the newspapers with praise; and as the Humane Society bestowed a medal on him, the incident directed attention to his exertions and character.

Not long after commencing the practice of his profession, Mr Antony Todd Thomson entered into the matrimonial state with Miss Christiana Maxwell, a lady belonging to a respectable family in Dumfriesshire.

In Chelsea Mr Thomson continued for the space of twenty years, diligently engaged in the performance of all those laborious duties which fall to the lot of the general practitioner.* To these, however, he did not confine himself. A person of the mental activity of Mr Thomson was unlikely to do so. His habits and occupations as a general practitioner led him to direct attention first chiefly to the departments of Pharmacy and Materia Medica, and the social position of a body at that time numerous and increasing, and daily acquiring more importance, namely, the Apothecaries.

His first avowed literary undertaking was the *Conspectus Pharmacopœiae*, published in the year 1810, a work which, from the large amount of useful information condensed within a small space, speedily recommended itself to the wants of the profession. The copyright of this small performance he sold to the late Mr Underwood for twenty pounds; and in 1833, when Mr Underwood retired from business, it was bought by Messrs Longman and Co. for the sum of two hundred pounds.

In the year 1811 Mr Thomson extended his labours in this department. He published the London Dispensatory, a work which in no long time shewed its utility by its extensive circulation in this country, and translations of it which appeared in different countries of Europe.

Mr Thomson also during this period conducted, along with Dr Burrows, the management of the Medical Repository, contributing to this Journal various essays, some on the necessity of improving the education and raising the standard of attainment among the Apothecaries. At length, with the aid of Dr Burrows and one or two more patrons, he succeeded in getting the Legislature to attend to this subject; and in 1815 was passed the first Apothecaries' Act, embodying the Apothecaries of England and Wales, and giving them the right to examine those who were in time coming to practise as Apothecaries, and to judge of their competency.

Mr Thomson evidently recognised the defects of the London system of education at the time, to which we refer. Though nowhere were there more able teachers of anatomy and surgery, and nowhere more ample means of clinical instruction than the large hospitals afforded, yet it appears that lectures on such subjects as Botany, Materia Medica, and

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* There is here some anachronism, which, however, we cannot venture to correct in any other mode than stating the fact as given to us. It is said that Dr Thomson continued in Chelsea twenty years; and that he left Chelsea in December 1826. If so, he must have commenced business in Chelsea in the year 1806. Yet this is not specified; and while, on the other hand, nothing is said concerning the time between 1799 or 1800, it is left to be inferred, that he had commenced in 1800 somewhere, though not mentioned.

Pharmacy, were either not in existence, or were incomplete. Dr Thomson is said by his editor to have been the first person who delivered lectures on Botany to a class of medical auditors in this country. By this must be meant England and London; for it is well known that excellent lectures on Botany had been for a long series of years delivered in Edinburgh, first by Dr Alston, then by Dr John Hope, and at the very time referred to, we suppose about 1816 and 1817, by the late Dr Rutherford.

At all events Dr Thomson, while still resident at Chelsea, and carrying on the business of a general practitioner, delivered in the room formerly occupied by Mr Joshua Brooks, the teacher of Anatomy in Blenheim Street, Oxford Street, a Course of Lectures on Botany. The delivery of these lectures led him to devote great attention to the study of Botany and the elucidation of Vegetable Anatomy by means of Drawings; and the researches into which he then entered induced him to determine on publishing Lectures on Botany, with illustrations. This must have been no easy task in those days, considering the small number of gardens, the few who had access to the large establishment at Kew, and the difficulty of obtaining rare plants and flowers in the neighbourhood of London. The Horticultural Society was not then in existence. There was no Botanic Garden. The only garden of any scientific value is said to have been that of the Countess de Vannes in the Bayswater Road; and in this spot, with the late Mr Loudon, Dr Thomson was in the habit of spending many hours every week. With all these obstacles and drawbacks, nevertheless, the energy and perseverance of Dr Thomson extended successfully.

With such aids as those mentioned, the Lectures on Botany were composed during the years 1820 and 1821; and though, in consequence of its want of success, the work was never completed, yet the merits of those Parts which had been published, were the means of making known the qualifications of the author, as a scientific inquirer and instructor. He thereby became known to Decandolle of Geneva, Dr Martius of Munich, and other distinguished persons abroad.

In the course of practice among the lower classes of Chelsea, Dr Thomson had occasion to see many examples of Diseases of all sorts, and, among others, of Diseases affecting the Skin. In this he felt his own deficiencies, and devoted himself to the removal of them with his wonted energy. By his influence with several of the wealthy and benevolent among his patients at Chelsea, a Dispensary was formed in the parish. To this Dr Thomson was appointed one of the medical officers, an arrangement which at once enabled him to be highly useful to the sick poor, and to cultivate that department in which he felt great interest.

In the month of December 1826, Dr Thomson left Chelsea, and commenced practice in Hindford Street as a licentiate of the College of Physicians of London. The same year, the University College of London was established; and in this institution Dr Thomson was appointed Professor of Materia Medica.

For this office Dr Thomson was extremely well qualified; and while he made great exertions to teach the elements of the science in an efficient manner, it is agreed on all hands that his success was proportioned to the efforts made. He made a valuable collection of objects in Natural History and Botany, with numerous drawings and diagrams, in order to illustrate the subject taught in all its divisions.

Not long after the commencement of his lectures on Materia Medica, a class for teaching the elements of Medical Jurisprudence was formed; and in 1832, Dr Thomson was appointed to lecture on the subject in conjunction with Mr Amos, to whom, as an able lawyer, were assigned the legal branches of the subject.

It is well known that the early career of the London University College sustained not a little discredit by various discussions and other circumstances unnecessary here to be recounted. Mr Horner retired from the office of Warden, and Sir Charles Bell resigned that of Professor of Surgery. The position of the Professorship of Anatomy gave rise to not a few unseemly disputes. In short, in 1832, of all the original professors, only Dr Edward Turner, Dr Davies, and Dr Thomson remained; and it is stated that it was chiefly by the exertions of these associates that the College struggled through this dark period of transition and trial, and came at length upon more hopeful days.

In 1829, Dr Thomson undertook the duty of editing the Synopsis of Cutaneous Diseases by Dr Bateman, and illustrated the text by a valuable collection of drawings of the most important forms of cutaneous disease. About the same time, he contributed several instructive articles to the Cyclopædia of Medicine; and in 1832, he published his Elements of Materia Medica. The lectures on Medical Jurisprudence were published in the *Lancet* in 1836 and 1837.

With all these literary labours, he continued to prosecute with great zeal the study of Botany, Chemistry, and Chemical Pharmacy; and in the course of his labours, he discovered a new, simple, and inexpensive method of producing corrosive sublimate. He also introduced as an active and serviceable remedy, the use of iodide of iron.

It could scarcely be expected that a life of literary and professional labours so unremitting, could be continued without leaving traces of its operation on the frame and health of the individual. In 1835, the health of Dr Thomson broke down considerably under the pressure of continued mental exertion; and during the same year, a serious and dangerous attack of illness was induced by inhaling chlorine gas, while lecturing at University College. The chemical irritation was followed by inflammation of the windpipe; and it was deemed proper that he should attempt travelling abroad for many weeks, with the view of recovering his general health. He joined, accordingly, a party of female relations and friends, and passing through Belgium, visited Switzerland and part of Holland.

From this tour Dr Thomson derived great benefit. His health was certainly improved; and he was enabled to resume his academical duties the ensuing winter, with considerable energy. By this time the North London Hospital was established; Dr Thomson was one of the physicians, and had the duty of delivering clinical lectures ôn the medical cases at the College. The restoration effected in the manner now mentioned, must have been considerable, as Dr Thomson proceeded in the performance of his various duties for the space of thirteen years after this period.

In the year 1838, Dr Thomson lost, after a few days' illness, his eldest son, Dr Alexander Thomson, by his first marriage. This young person had received a comprehensive medical education, in Edinburgh, London, and Paris; and, as he gave promise of being an able physician, the loss was very severely felt by his father. Dr Thomson, however, continued his active and industrious course. In 1841 he published the Domestic Management of the Sick Room, a small volume, containing many useful precepts and directions, and which in no long time became extensively known. Of this work two editions, comprehending two thousand five hundred copies, were published.

Meanwhile, his merits as a scientific cultivator of medicine were recognised in December, one thousand eight hundred and forty-one, by his being admitted to the fellowship of the College of Physicians, shortly before the resignation of Sir Henry Halford as president.

In the autumn of 1848, Dr Thomson's family perceived in his appearance marks of a decline in health and spirits; accompanied with loss of appetite, and a greater degree of fastidiousness as to food than he was wont to shew. He resumed, nevertheless, his duties at University College, and delivered the opening lecture with all his wonted animation. On

Christmas day, when, for the time which proved to be the last, he assembled his relations, according to custom, around him, the first manifest symptoms of illness appeared. About one week after this he dined with an old friend and patient. He had sent home the carriage, and walked to the house of his friend, where he sat in damp boots. An attack of *bronchitis* ensued. This was checked by the skill and attention of his friend, Dr Watson, and of his nephew, Dr Parkes. He appeared for a time to revive; and it is supposed that, could he then have been persuaded to quit London, and visit a warmer climate, his life might have been for a few years prolonged. No persuasion, however, could induce Dr Thomson to try any more effectual change at that season, than a temporary removal to Hampstead. During a second visit to that place, he was so far recovered as to be able to walk about upon the Heath, and to sit on a bench for a few moments in the sunshine of the spring. He at this time expressed the conviction that he had not six months to live.

He returned to London, and exerted himself to renew his usual occupations. A visit to Rothamsted Park, the seat of Mr Lawes, well known as an agricultural chemist, seemed to be the means of again recruiting his strength. For one day more he resumed his duties at the North London Hospital. While there, signing certificates for the pupils, he was seized with a shivering fit, which, however, he disregarded, and proceeded to perform a professional visit at Lambeth, in conjunction with Dr James Young. While writing a prescription he fainted; and though he revived and finished the prescription, the disease, which had been fastening upon him, inflammation of the lungs, was established.

This attack, nevertheless, was prevented by the judgment and attention of his medical attendants, from proving immediately fatal. But it must have left the lungs in a greatly injured state. Dr Thomson survived to take the communion on Easter-day, 1849; and afterwards to remove to Ealing to gratify his longing for the country. In this place he breathed his last on the third of July 1849, in the seventy-second year of his age.

In compliance with the wish that he had several times expressed, his mortal remains were interred in the quiet country churchyard of Perrivale, near Ealing, followed to the grave by the immediate members of his own family, four professors of University College, and one or two friends. The students of the College requested permission to attend the ceremony; but as it was wished to be private, this was respectfully declined. A few, nevertheless, spontaneously went to witness the last mark of earthly attention to their former instructor.

Dr Antony Todd Thomson, though naturally a man of firm and elevated moral principle, was fully convinced of the necessity of sound religious faith in regulating the conduct of men in the present life. In the truth of revelation he was a sincere believer; and throughout his whole life he never lost sight of the necessity of regulating his conduct by the principles, which he professed.

Dr Thomson appears to have been a person of great benevolence and kindliness of disposition. By those who knew him, he was at once beloved and esteemed. An interesting story of his grateful attention to a friend of his youth is recorded. It has been mentioned that Dr Thomson began life as a clerk in the Post-Office at Edinburgh. At that time the late William Kerr, Esquire, who held the office of secretary, treated Mr Thomson with great kindness and indulgence. Many years afterwards, when Mr Kerr became indigent, he found a home for nearly two years in the house of the man to whom he had been kind, at a period when kindness was most serviceable.

It was the desire of Dr Thomson that the Museum of Materia Medica, in forming which he had bestowed so much labour and spent so much money, should be acquired by University College. But this was declined from want of funds. The Museum was subsequently purchased by Government for Queen's College, Cork, where it is preserved under the care of the professor of Materia Medica; the name of the collector being retained.

Two daughters of the first marriage survive. By the second three sons and five daughters were left.

1. The Practical Treatise on Diseases affecting the Skin was the last professional work in which Dr Thomson was engaged. It appears that he lived not to complete the work; a duty which, we are given to understand, was performed by his nephew, Dr Edmund Parkes. At the time of his death three hundred pages had been printed, nearly two-thirds of the work.

In undertaking a work like the present, Dr Thomson had well qualified himself, both by his intimate practical knowledge of the subject, and by his previous edition of the Synopsis of Dr Bateman.

The arrangement differs in several respects from that given by Willan and Bateman; and it must be allowed that, as Dr Thomson has taken a more comprehensive view of cutaneous disorders than these eminent Dermatographers, the order in which cutaneous disorders is here presented, is probably better suited to the ideas of modern practitioners. The work consists of Ten Chapters and an Appendix.

Dr Thomson arranges the whole family of Cutaneous Disorders under ten general heads.

The First consists of the Febrile Contagious Diseases, not taking place more than once in the course of life, unless in exceptional cases. This is a large division, and embraces all the acute febrile contaneous affections, including Typhus Fever, after the manner of Hildenbrand.

These eruptions are, Scarlet-Fever, Measles, Small-Pox, Chicken-Pox, Cow-Pox, Yaws and Sivvens, and Spotted Typhus and Typhoid Fever.

The Second Division is that of Febrile Contagious Diseases recurring several times during life, and contains only St Anthony's Fire or Rose.

The Third Division which is entitled Non-Febrile Contagious Diseases, embraces Scall-head and Itch.

The Fourth Division, designated Non-Contagious Febrile Diseases, includes, Erythema in all its varieties, Nettle-Rash, Rose-Rash, the Miliary Eruption, Pompholyx and Pemphigus.

The Fifth Division consists of Cutaneous Diseases, Non-Contagious, Non-Febrile, chiefly connected with, and indicating the presence of, Derangements in the Digestive, Assimilating, and Secreting Organs.

This forms a large division, and embraces the following Orders.

1st. The Papular Eruptions, Lichen, Strophulus and Prurigo; 2d, the Pustular Eruptions, Impetigo, and Ekthyma; 3d, the Vesicular Eruptions, Herpes, Ekzema, and Rupia; 4th, the Scaly Eruptions, Psoriasis, Leprosy, Pityriasis; 5th, Tuberiform or Tubercular Eruptions, Lupus, Elephantiasis or Arabian Leprosy, and the Aleppo Disease; and 6th, Hæmorrhagic Diseases of the Skin, for instance Petechiae, Vibices, Purpura, and Scurvy.

The Sixth Division receives three Cutaneous Disorders, which are of doubtful position, and are not included in any of the former; namely, Pellagra, Ichthyosis, and Pachydermia.

The Seventh Division contains the Eruptions supposed to be connected with Syphilis.

The Eighth Division, which includes what the author calls Non-Eruptive Morbid States of the Skin, treats of the Hypertrophies and Excrescences, the abnormal colorations of the Skin, the abnormal states of the Sudoriferous Glands, and the Sebaceous Follicles, Acne, Sycosis, and morbid states of the Hair Follicles and the Hair.

The Ninth Division, entitled New Formations of the Skin, contains the Description of the Cheloid Disease, Lipoma, Telangiectasis, Wens, Molluseum, Warts, and Condylomata.

The Tenth Division consists of morbid states of the Nails.

In the Appendix are given various pieces of information on subjects not fully, or not at all considered in the several heads now mentioned.

It is not difficult to perceive that this arrangement though comprehensive, is not by any means perfect, and that it is open even to not a few objections. It is unnecessary, however, here to examine it in a critical manner. Our duty we conceive to be accomplished, when we shew the general manner in which the author treats the subject.

At the beginning of each division, Dr Thomson explains in a general manner, the physiological and pathological relations of the cutaneous affections considered. The general and particular descriptions are given with care. Each subdivision is illustrated by the recital of appropriate cases. In short, the author seems to have been anxious to give the student as correct views as possible of the mutual relations and distinctive characters of this class of diseases. The part of the work which is most elaborate, is, as might be expected from one who had devoted so much of his time and attention to the departments of Materia Medica and Pharmacy, that relating to treatment, the administration of internal medicines, and the employment of external applications. Dr Thomson has here collected all the remedies that have ever been used in the treatment of cutaneous diseases; and the reader may be assured that scarcely any serviceable medicine or external application has been omitted. Probably a little more selection, and a little more precision, might have been desirable; certainly it would have been advantageous. But this can scarcely be imputed as a fault to the author or his editor. It is the evil of the subject, that for many cutaneous disorders there are multiplied means of treatment, numerous applications, but almost no certain remedies; and too often, the physician, after finding that one means which had previously in other cases been effectual, proves unavailing, is under the necessity of making a series of trials and experiments, with new remedies, several of which may be equally unserviceable as the others. The treatment of diseases of the skin, is, in short, still very much in an empirical condition, excepting as relates to the acute febrile eruptions, in which nature and constitutional powers do much, and medicine does very little.

From the short notice now given, it will be natural that we say that this work must be one of great service to the medical practitioner; and if he do not always get in it the exact treatment which may prove successful, he will at least find judicious information and good suggestions, which may put him in the way of curing some very unmanageable cutaneous disorders.

2. The Conspectus of the British Pharmacopœias is a book so well known, that it is quite unnecessary to speak of it in the ordinary manner. The present is the sixteenth edition, a fact which shews how completely this small performance has obtained the confidence of the practitioners of Great Britain. As the work, notwithstanding its merits, must have fallen behind since the death of the author, had means not been adopted to prevent this result, the present editor, Dr Birkett, has done everything which the progressive advancement of Materia Medica and Pharmacy required, in order to provide the junior members of the profession with a safe and instructive guide, for administering remedies and preparing medicines according to the most approved principles.

It may not be improper to mention that fourteen thousand copies of this useful Manual have been published since the year 1831; and that the present is the second edition since the death of the author.

3. The London Dispensatory is a work not less extensively and not less favourably known than the Conspectus. It is a more comprehensive work, more scientific, more elaborate, and of greater pretensions. When it made its first appearance in 1811, it speedily supplied a want which must have been long felt by the practitioners of England and Wales. The Dispensatory of Dr Lewis had been the one long in use there; but after the death of the author, the work was not continued or made to keep pace with the progress of chemistry and pharmacy. In this part of the island, indeed, this duty was accomplished with unequalled skill and very correct chemical knowledge by Dr Duncan junior, whose work, as was formerly stated in volume fifty-seventh, had gone through ten large editions in rather less than twenty years.*

On the model of this excellent work the London Dispensatory by Dr Thomson was composed; and it is scarcely a matter of doubt, that to this circumstance, as well as to the ability and correctness with which the work was prepared, it owed a large amount of its success.

Like its Edinburgh prototype, the London Dispensatory consists of three Parts or General Divisions.

The first part is a short treatise on the Elements of Pharmacy, subdivided into three sections; the first, on the Physical and Chemical properties of Bodies, Attraction, and Repulsion; the second, on the Constitution of Bodies; and the third, on Pharmaceutical Operations and the Apparatus required for accomplishing these operations.

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* Edinburgh Medical and Surgical Journal, Volume Fifty-seventh, p. 502. Edinburgh, 1842.

The second part is devoted to the subject of Materia Medica. The articles are arranged as usual in the alphabetical order; and under each is given an ample and clear history, commercial, natural, therapeutic, and pharmacological of each article, and short notices of the preparations into the composition of which it enters.

It is only justice to say, that this part of the work, which was in the peculiar department of Dr Thomson, has always been regarded as done with great ability and shewing much accurate knowledge of the subject. It was a department to which he had early and long devoted great attention; as it was written with the hand of a master, its merits have been proportionately recognised.

In the present edition this division loses nothing of its original value. Dr Garrod, who succeeds Dr Thomson in the office of Professor of Materia Medica in University College, is known to be well qualified for this duty, and has every inducement to perform it in a creditable manner. Dr Garrod shews throughout intimate knowledge of the subject, and has presented to the practitioner a correct and serviceable Dictionary of the Materia Medica.

Dr Garrod has been chiefly desirous to carry forward the chemical and pharmacological qualities of the work, so as to make it throughout exhibit a just view of the present state of knowledge on these subjects. All matters of doubtful utility and all controverted points no longer interesting to the present race of practitioners have been omitted. In one respect Dr Garrod has not felt it expedient to introduce any change. This relates to the therapeutical doctrines and principles delivered by the author, which he justly thinks ought in general to stand unmodified, as they were left by Dr Thomson. As these form important characteristics of this division of the work, the editor must be allowed to have acted judiciously in the meantime at least.

The Third Division of the London Dispensatory consists, like that of Dr Duncan's Edinburgh Dispensatory, of a Translation of the London Pharmacopœia and Reprints of the Formulæ of the Edinburgh and Dublin Pharmacopœias, with explanations and commentaries, chemical and pharmacological, upon the processes recommended in each.

This part contains all the officinal preparations of the London, Edinburgh, and Dublin Colleges; and as the appearance of the new Pharmacopœias has rendered necessary various changes and corrections, all care has been bestowed in rendering this division of the work as accurate as possible.

From the circumstances now mentioned, as well as from the intrinsic ability with which these explanations and commentaries are given, this part of the work ought to be perused with great interest, and it would be idle to doubt, with not less advantage.

Besides the Appendix to the Pharmacopœia and the Tests, the work contains two proper Appendices, which convey a large amount of useful information.

The First Appendix consists of Seven Parts; the first, on Waters, and the method of Analyzing Mineral Waters; the second, on Electricity; the third, on the art of prescribing Medicines; the fourth, Tables of Incompatible Articles; the fifth, Examples of Extemporaneous Prescriptions; the sixth, a Posologicable Table; and the seventh, shewing the amount of Opium, Arsenic, Mercury, &c., in several important medicinal preparations.

The Second Appendix consists of various Tables useful for reference and consultation.

In conclusion, it may be said, that the present edition is well calculated to sustain the well-founded character of the London Dispensatory.

III.—*The Principles and Practice of Surgery. Illustrated by numerous Engravings on Wood.* By WILLIAM PIRRIE, F.R.S.E., *Regius Professor of Surgery in the Marischal College and University of Aberdeen, Surgeon to the Royal Infirmary.* London, 1852. 8vo, pp. 952.

This work deserves to be made generally known, in consequence of its remarkable merits as a Treatise on the Principles and Practice of Surgery.

It consists of twenty-five chapters, in which the following subjects are successively treated:—Inflammation and its Results, Erythema and Erysipelas, Wounds, Burns, Fractures, Injuries of the Head, Dislocations, Affections of the Osseus System, Diseases of Joints, Curvatures of the Spine, Talipes or Club-Foot, Affections of the Arteries and Veins, Hernia, Wounds of the Abdomen, Calculous Disorders, Affections of the Testicle, Affections of the Genito-Urinary Organs, Amputations and Resections, Deligation of Arteries, Diseases of the Rectum, Diseases of the Eye and its appendages, Diseases of the Nose, Diseases and Injuries of the Mouth, Throat and Windpipe, Tumours and Diseases of the Breast.

It would be impossible to examine, even in a superficial and cursory manner, a list embracing so many diseases and injuries of the human body. But it is due to the author to say, that he has been at great pains to place before the reader faithful descriptions of surgical diseases and injuries, and to explain upon the most approved principles the treatment required by each. The accounts of the several operations are given with clearness and ability.

The work is highly creditable to the knowledge of the author, and cannot fail to be a most serviceable guide to the student and practitioner of surgery.

ART. IV. — *Observations on Surgery.* By BENJAMIN TRAVERS Junior, F.R.C.S., lately Resident Assistant-Surgeon at St Thomas's Hospital, and Lecturer on Surgery, &c. &c. London, 1852. 8vo.

The Observations contained in the present volume relate to the following subjects :—I. Fracture of the Cylindrical Bones; II. On the Local Treatment of Fractures, and on the General Treatment of Fractures, simple and compound, illustrated by cases; III. The remote consequences of Fracture; IV. Injuries of the Head, and Injury of the Substance of the Brain; V. Puncture of the Bladder; VI. Several Disorders of the Male Urethra; VII. Certain forms of local inflammation, as Boil and Carbuncle, with Burns; and VIII. Diseases of the Joints.

I. Of Fractures, four forms are observed; 1st, the transverse; 2d, the oblique; 3d, the comminuted; and 4th, the impacted. Besides these common forms of injury, bones are liable to be split or fissured longitudinally. Petit used to deny that a bone is ever split. Duverney, however, had previously entertained no doubt of the possibility of this sort of fracture, and he quotes three instances of it in his Treatise on the Diseases of Bones. These observations were not regarded as conclusive; and the opinion of Petit has prevailed more or less generally among surgeons. This opinion was maintained by Petit chiefly on speculative grounds. He argued, that there is no point of a blow capable of breaking a bone in a longitudinal direction, which could not with much greater facility break the bone transversely or obliquely. Boyer remarks, that supposing this fracture really had taken place, it would be impossible to recognise its occurrence in the living body through the soft contused parts by which the bone is covered, and to distinguish the effects of it from simple contusion. This last reason, it may be observed, is away from the question. An injury or a lesion may be perfectly possible, and may actually have taken place, though the means of art or the observation of the surgeon are inadequate to discover the existence of either. Boyer is obliged, nevertheless, to allow, that sometimes in consequence of gun-shot wounds, bones are found to be cloven or split even into the articulations. But these occurrences, he adds, do not prove the possibility of simple longitudinal fracture or fissure of a bone.*

On this point the testimony of Mr Travers deserves to be made known. Mr Travers on one occasion dissected a limb, in which the shell of the tibia on its cutaneous aspect was split nearly from end to end ; and surgeons are familiar with the accident of detached or split processes, as Condyle and Trochanter. In the case now referred to, the exact nature of the injury was not apparent before death, which was not immediate. The fissure extended into the ankle-joint, this cleft terminating above in a transverse fracture of the bone about four inches from its head. The injury was the effect of the passage of a waggon-wheel across the limb in an aged man. No attempt at adhesive action had been instituted throughout the tract of the fissure. The patient remained well until within forty-eight hours of his decease. He sunk somewhat suddenly, but had no fever or other severe constitutional disturbance indicative of irreparable injury.

Mr Travers adds, that he saw at St Thomas's Hospital, under the care of Mr Green, a fracture of the collar-bone, which might have been called a fissure. In this case there was a cleft extending between the two curves of the bone, and occupying all its central or straight portion. The patient did well.

In the Museum of St Bartholomew's Hospital is preserved a fragment of an ulna four inches long, and which was split off from the body of the bone by machinery. Here the fracture was strictly longitudinal.

In the Hunterian Department of the Museum of the College of Surgeons is a specimen of fissured *Tibia*, where the direction of the fracture is precisely that of the long axis of the bone.

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* Traité des Maladies Chirurgicales et Des Operations qui Leur Conviennent. Par M. Le Baron Boyer. Tome Troisième. Seconde Edit. Paris, 1818, p. 10.

In the spring of the year 1850 Mr Travers sent to the College collection a specimen of split humerus from the horse, attended by comminution of the scapula of a most extensive description. The accident occurred in the field from a fall in leaping over a fence. The animal alighted upon an uneven surface, and crossed his forelegs in coming to the ground. The head of the humerus has undergone a perpendicular cleavage, such as might have been produced by the blow of an axe.

In the same collection are specimens shewing how grape-shot and musket-balls, striking the shaft of a cylindrical bone as the femur, effect a perpendicular fissure into the nearest joint.

This brings the question to the condition in which it was left by M. Boyer. There is, indeed, no circumstance either in the nature and relations of bones, or the forces by which they are struck, to render fissure impossible. All fractures depend as to their nature and duration upon the structure and figure of the bone; upon the direction, force, and velocity with which the agent strikes the bone, and upon the action of the attached muscles. When an agent directly strikes a bone with great force, or is impelled against it with great velocity, it produces compound fracture, not unusually comminuted, and may, if the force be confined to one point, cause a fissure. Where, on the other hand, the agent is either not forcibly impelled, or indirectly strikes the bone, it may either cause a simple oblique fracture, or with the resistance of the muscles it may do so. When, in the third place, a cylindrical bone is forcibly struck by an agent impelled with great velocity, as a musket or pistol ball, though the bone is very likely to be broken transversely or comminutely, it is also likely, as shewn by such cases as those above mentioned, to be fissured longitudinally. In fact, cylindrical bones are easily split by direct forces in the dead body; splinters are almost invariably longitudinal, shewing that in the long direction their fibres are separable; and it is not easy to see, even on theoretical grounds, why cylindrical bones may not be split and cloven longitudinally.

Mr Travers is of opinion that split bones should form, in the catalogue of fractures, a distinct order. These fissures may be simple; but they are more commonly combined with comminution; they have also been observed where the bone is broken transversely. The splitting may terminate at one end upon the exterior of the bone; but Mr Travers regards it as essential to the accurate use of the term, that the other terminus of the fracture should be within the circumference of the bone, or upon its synovial surface. Such an application of force as detaches a fragment after the production of fissure in the first instance, as happened to the specimens preserved at St Bartholomew's Hospital, invalidates not this general rule, that case being in its nature an exception.

Upon the history of these split bones, Mr Travers makes two practical remarks. *First,* In the majority of instances, the mischief is prolonged into a joint, generally of large dimensions, as the elbow, the knee, the ankle. *Secondly,* Split bones do not unite. This remark applies not to fissures of the cranium. The circumstances are not the same. But when the shell of the large cylindrical bones is extensively fissured, as here described, bony union follows not. The author allows that this is an opinion,—he means an inference,—which depends in part upon observation, and partly upon reasoning. Some of the causes of the failure of union, he suspects to be ; 1*st,* that split bones occur for the most part in aged persons ; 2*d,* the cancellated structure is more injured and to a greater extent, as is shewn by effusion of blood into the broken cells, than takes place in ordinary forms of fracture ; 3*d,* there is an indisposition, connected with incapacity, either to institute or promote adhesive inflammation ; and 4*th,* the periosteum is not unfrequently separated from the bone, to an extent incompatible with its office as the source and *nidus* of new bone.

In speaking of the local treatment of fractures the author considers the respective merits of the wooden-splint, the gum-fastening, the Paris-plaster-splint, the lined leather-splint, and the gutta-percha supports. He speaks in most favourable terms of the common wooden splint as being almost always at hand, and generally applicable ; the lined leather-splint for particular fractures, those for instance of the fibula ; and the gutta-percha casings for the bones of young children.

Some judicious observations are made upon constitutional treatment. The great duty is to watch the condition of the patient, to be vigilant, but not to be active with medicine. Hepatic obstruction and torpor are the great evils against which it is requisite to be upon the guard. Diarrhœa is a dangerous symptom, and care should be taken not to induce it by the administration of irritating cathartic medicine. The practice of blood-letting in the treatment of fracture is extinct in the metropolitan hospitals.

When at an advanced period of the treatment, the system becomes languid, the form of support may be diffusible and such as rapidly reaches the capillaries. The English of this is, that brandy and water is to be allowed, especially if the patient have been addicted to the use of strong and spirituous potations. When medicine is required it is generally opium, in order to allay irritability; and for this purpose the old preparations of the drug, in an adequate dose, are as effectual as the salts of morphia.

As to any other medicinal agents, quicksilver, especially the blue pill, or the gray powder, with a few grains of rhubarb, or scammony, are the most eligible.

In conclusion the author recommends that the constitutional treatment to be safe must be deliberate; that is, we understand, carefully considered and adapted to the condition of the patient. If he be restless and constipated, purgative medicine is indicated; if irritable and sleepless, opium must be given. Always abide and be guided by the symptoms.

The following case is given to shew the sudden and rapid mode in which dissolution sometimes takes place after fractures, or rather in the course of treatment.

A gentleman, aged thirty-three, of cool and temperate habit of body, sustained a fracture of both bones of the leg above the ankle-joint. On Friday, the 4th of November 1842, exactly one month after the receipt of the injury, a friend was sitting with him till half-past nine at night. He was in high spirits, sang a song, and expressed great delight at the prospect of being on the sofa in a day or two. At a quarter before eleven he cried out to his bother that he was in severe pain, desired that the pillow on which the limb was resting might be shifted, and that an opiate might be given to him immediately. His brother was busily occupied in preparing the draught, when he exclaimed, "Bless me the pain is dreadful; I feel it all across, and from top to bottom." He then gave a start, breathed heavily, and was convulsed. Presently afterwards he swallowed some brandy, and said that he felt as if dying. The convulsion returned, when more brandy was offered; but he could no longer swallow, and almost instantly was lifeless.

The body was carefully examined by Messrs Esther, Lowe, and Prichard of Bristol; but no adequate cause of death could be detected. On examining the broken surfaces of the bones, it was seen that the process of union had been very slow and imperfect, considering the time which had elapsed since the date of injury.

In injuries of the extremities, believed to require amputation, Mr Travers observes that it is often difficult to decide, what amount of suffering the constitution may endure in the way of operation. If, for instance, the injury be so extensive as to require the amputation of both legs, or an arm, in addition to amputation of one of the lower extremities, the chances are greatly against ultimate restoration. If the patient dies not immediately, the exhausted system proves unequal to the task of repair. Febrile reaction, with sloughing of the stump, tetanus, or a pure progressive collapse of the vital powers, determine a fatal result in the great majority of cases, and that at no distant time.

Loss of blood also contributes in some constitutions very much to these unfavourable terminations.

"Two men were riding upon the shafts of a waggon, when the horse became unmanageable, and they were precipitated beneath the wheels. James Saunders, ætat. 48, hale and florid, sustained a simple fracture of the right thigh, also compound fracture of the left thigh and leg, attended in the latter case with comminution and extensive laceration. George Tilt, ætat. 35, of a meagre aspect, escaped with a bad compound fracture of both bones of the left leg. There were no means of ascertaining the quantity of blood lost in either case; it was thought not to have been very large. The accident happened in the Kent Road. In the first instance they were carried nearly two miles to the Deptford Dispensary, and thence brought back to St Thomas's Hospital. The first man soon rallied, and the left limb was removed in the upper third of the thigh, shortly after his admission, with his full consent. The femoral artery was secured before making the ordinary incision, but during the operation a gush of arterial blood took place on dividing the profunda. He lost nearly a pint of blood with great rapidity; when he became suddenly very faint, and died upon the table almost without a struggle. He was examined; the viscera were in all respects healthy. The second man, Tilt, who had been much reduced by poor living, lay in a state of extreme exhaustion all night, having been admitted at half-past four o'clock on the previous afternoon. Extremities cold—expression fixed and glassy; indeed he appeared to be on the verge of dissolution. He was supported through the night chiefly by small quantities of brandy at intervals, and on the following morning he took some aromatic spirit of ammonia with camphor. In the afternoon he also had rallied sufficiently to bear the operation below the knee. The limb was removed by Mr South, and the amount of blood lost was inconsiderable. The man remained in a precarious condition for some days afterwards, but the report speaks favourably of him as to expression, rest, and appetite. He recovered perfectly, and left the house in the beginning of May.

The first of these cases clearly illustrates the effects of accumulated injury. 1. Fracture of both limbs; the left broken in two places, and compound in both. 2. The necessary loss of blood. 3. The journey backwards and forwards, exceeding five miles in all. 4.

Ligature of the femoral artery. 5. Amputation, which might have proved successful but for the accident of renewed hæmorrhage from a large arterial trunk during its performance.

There can be no doubt that the first operation was a measure warranted by the results of experience: but should there be any hesitation upon the reader's mind as to that point, we refer him triumphantly to the event of the second case. Here, in the face of every discouragement, a like bold procedure was crowned with success, and its principle amply vindicated."—(Pp. 51–53.)

Under the head of Injuries of the Brain are considered the two subjects of Concussion and Compression. To the former the author applies the French term Commotion, in our opinion quite unnecessarily. The usual term of Concussion has been long in use, and is well understood, and need not be given up for that employed by French surgeons. If there be any reason for discarding the old term and adopting a new one, we should prefer either the English term Stunning or Shattering, or if a foreign one must be had, the German one Erschütterung; which has the advantage in some degree of suiting the word to the sense.

Mr Travers suggests the following arrangement of injury of the head as more simple and descriptive of its several forms, than any hitherto propounded for general purposes of practice and observation. All these accidents may be regarded as belonging to one of two principal divisions, which may be termed (1.) Commotion; (2.) Compression. Irritation and inflammation of the organ can scarcely, he thinks, establish a claim to a separate divisional classification. These are rather remote consequents or incidents, and grow out of the peculiar circumstances of an individual case. As to their frequency, inflammation is more rare than convulsions, paralysis, or other purely neuralgic conditions, and especially so in recoverable instances.

Commotion is either fatal,—or transient and remediable by the means of art. It embraces all cases between the two extremes of shock or blow, which kills immediately, leaving no visible disintegration or trace of its effect after death; and those examples of protracted reaction, the event of which is uncertain through a period of many days. To the head of Commotion, the author refers equally simple stunning, which is followed by speedy recovery; and those mixed conditions of alternating stupor and excitement, which terminate fatally after an uncertain interval, and depend upon lesion of the substance of the brain. The last are often attended by so many signs of recoverable shock, as to excite doubts about their actual nature, until the patient begins to die. To this order belong laceration of the brain, and fracture of the base of the cranium, in all its forms.

Compression, whether by blood or by bone, is recognised as occurring in one of two forms.

1. It is irremediable either from its extent or its intensity, or from the combined operation of both circumstances.
2. It may be relieved by removing the depressed and compressing body.

Compression remediable by means of art is of two kinds. It is either partial or complete.

When compression is partial, it is denoted by signs of irritation. Of this order are spiculated growths, angular projections, and some of the fractures of the internal table.

The other, the complete variety, embraces all those cases of collapse, stupor, and insensibility, which shew that the lesion is at once persistent and complete.

The whole are presented in the following tabular form.

Commotion.

1. Fatal, without lesion or reaction of any kind. Rare.
2. Simple, with a short and healthy reaction. (Ordinary Concussion.)
3. Obscure, with slow reaction, and recovery more or less complete.
4. Lesion of the substance of the brain, with or without fracture of cranium, uniformly fatal in the latter, and ordinarily so in the former case.

Compression.

1. Directly fatal, whether from the nature or extent of the injury.
2. Not directly fatal, but remediable by art, and occurring in one of two forms.

a. Pure, by surfaces of blood or bone.

b. Partial, by depressed edges or spiculæ; the symptoms being incomplete, and marked by signs of irritation.

In whatever mode the action of the brain ceases, in consequence of interrupted connection, to exert its influence over the rest of the body, from that moment the animal so circumstanced begins to die. The condition of the brain may be that of disease, and under such circumstances the system exhibits signs of its altered function, so long as its connection with the spinal chord is preserved. But when the circulation in these organs is suspended, or their continuity is permanently interrupted, the process of dissolution commences, and is in shorter or longer time, according to the character and extent of the lesion, completed. For instance, in certain examples of local injury, or where chronic disease of an important organ has gradually impaired the proper nutritive powers of the system, life has been known suddenly to cease, and the flame has been extinguished almost in one moment. Without any premonitory sign, the patient complains of præcordial oppression, a sense of faintness ensues, then restlessness, he gasps and says he is about to die, and in truth expires perhaps upon the spot.

When the brain has undergone, either in the progress of disease or from external violence, changes which prevent the organ from longer exerting its just influence over the lungs and the heart, we are informed of the fact through the medium of the circulation. The pulse may be feeble, small, intermittent, and irregular, or it may be double its usual frequency and regular, or it may be flagging and unsteady; all symptoms in head injury which ought to be regarded with apprehension.

Mr Travers adverts to the old practice of using the trephine, as in the days of Dease and Pott, for all injuries of the head indiscriminately. Mr Pott, he thinks, mistook the true road upon an erroneous hypothesis of prevention, preventing in truth, what was by no means certain to take place. Mr Dease fairly admits that the people all died in spite of the trepan, physic, bleeding, and low diet, and that those who escaped the operation or any treatment at all, often made the best recovery.

And why did all the people thus treated die? Simply because they underwent, in addition to a good stunning blow upon the head, a most elaborate and systematic mechanical injury, which, exclusive of the blow and its effects, would have caused, and did cause, inflammation of the brain and its membranes. Let any one read the inspections of Mr Dease's cases with the light of morbid anatomy, and compare with them the similar and analogous cases and inspections given by Schmucker, the Prussian army surgeon, as to cases of wound and injuries of the skull, and he will be satisfied without further inquiry and further evidence, that the application of the trephine was almost invariably followed by inflammation of the brain and its membranes.

Such, however, was the practice at that time, the illuminated eighteenth century, even its latter half, that surgeons all over Europe were everywhere doing the same thing; all boring, chiselling, sawing, and hammering at the human skull, as if it were made of good fir deal board, or the shell of a cocoa-nut, and contained nothing more animated or more delicate, than cocoa-nut and its milky fluid. Desault and his predecessors had been in the habit of constantly performing the operation of trepan at the Hotel Dieu of Paris, for all fractures, real and supposed, and all injuries of the head in which there were stupor, convulsions, or any symptoms supposed to indicate fracture. Desault, and also various previous surgeons, had observed that the operation almost never succeeded; and he accordingly limited the use of the operation so far as to refrain from applying the trephine in fractures *without depression and without* effusion of blood. This practice, Boyer remarks, succeeded perfectly, and is generally followed at the present time. Accordingly, when a fracture of the skull is accompanied with depression, or an effusion of blood which compresses the brain, we apply the trephine in order to elevate the depressed fragments of bones in the first case, and to give issue to the effused blood in the second. But if there be neither depression nor effusion of blood, we do not apply the trephine. We confine our practice to the use of general remedies, such as bloodletting from the arm and the foot, laxatives, tartar emetic enemata, and all those means calculated to obviate irritation.*

So says M. Boyer of the practice of French surgeons; and doubtless this wise observation of Desault was productive of much benefit. In another part of his work M. Boyer expresses the opinion, that the unfavourable termination of the trepanning performances was owing to the bad, foul, vitiated air of the hospital. Doubtless this air, which was probably the most noxious presented to human lungs in Europe, did no small amount of mischief. But had the air been as pure as that on the Landes of Bourdeaux and on the mountains of Auvergne, which is the driest, purest, and most salubrious in France, the operation of boring and sawing a hole into the skull must have sent a considerable proportion of its subjects to the priest and the sexton. The man who thought of boring a hole through the human skull for curing the effects of either injury or disease must have been a most accomplished person; rarely gifted in physiology and pathology; a friend to the human race doubtless he was; and probably he will be discovered by some future inquirer into history and antiquities, to have been one of the main promoters of the first French revolution. Peace to his ashes.

Desault doubtfully recognised the features and merits of the Moloch of Operative Surgery. If he did so, he did it in a cold, matter-of-fact, accidental manner; and scarcely perfectly did he do so. A more acute, more original person, and to our mind one of a more comprehensive mind was John Abernethy. He it was, who saw at once what was wrong; at once, and, as it were, for ever pointed out the errors; and so perfectly lighted and beaconed the whole course, that surgeons could never wittingly fall again into the same mistakes, and commit the same number of homicides. His Essay on Injuries of the Head is a lasting memorial of the happy application of correct physiological and pathological knowledge with strong common sense, to rectify the errors of surgeons, and teach them a rational method of treating injuries of the head, By him they were instructed to discard mechanical means and instruments, unless when absolutely necessary, and to treat injuries of the head by medical measures upon physiological principles, and by observing carefully, and reflecting deliberately, upon the state and symptoms of the patient.

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* Traité des Maladies Chirurgicales, &c. Par M. Le Baron Boyer. Tome Cinquième, p. 74. Paris, 1818.

Mr Travers thinks, probably not without justice, that surgeons are in a degree still greater indebted to the surgical pathologists in our own day; and above all, to the originality of John Cunningham Sanders, of the London Eye Infirmary, in shewing the powerful effects of mercury in checking iritic inflammation. Probably, this is the case. Quicksilver, when employed in a cautious and discriminating manner, is a valuable, powerful, and serviceable agent; but it must not be forgotten that in certain circumstances it irritates, masks, and perverts symptoms, in a degree so considerable, that the characteristic features of the original disease or injury are either obscured or lost sight of. He forgets not to recommend bloodletting, and especially the slow oozing from twenty leech-bites. The latter, he says, is not only more effective, but by far the safest practice. This it would be wrong altogether to deny; at the same time, we have seen most excellent recoveries from the secondary effects of concussion under the use of bloodletting from the arm, practised to moderate extent according to its effects, and repeated according to the state of the symptoms.

The gradual progress, from irritation of the brain by these secondary effects, to a more healthy and natural state of the circulation, is well described, and shews that Mr Travers has observed his patients to some purpose.

Counter-irritation, as it is now called, that is, revulsion and derivation, is a good means of affording relief to the oppressed and irritated brain. But the time for this is after the prominent force of the symptoms has been broken by discharge of blood, and some derivation from the head by purgative medicine. The blister applied repeatedly and allowed to heal, not to be kept open, is as good as any other agent.

In speaking of Cathartics he disapproves of the immediate exhibition of large doses of calomel in simple concussion. This cholagogue is more eligibly given after the lapse of a few days when the bowels are torpid, and the liver loaded. But he fails not to direct attention to the lowering effect of mercurials when given in full doses. It seems strange that Mr Travers disapproves of, or deems unnecessary, the employment of Enemata in the treatment of these injuries. Scarcely, we must say, is any form of remedy acting upon the alimentary canal so effectual, so beneficial, so free from the charge of doing mischief. They may be made purgatives without the extensive irritation of these medicines. They are, in all instances, eccoprotics, and consequently remove hurtful irritation; and they are derivatives, and withdraw from the head the current of circulating fluid.

In certain instances of injury of the head it happens, that, after the lapse of several days from the date of the injury and the development of the symptoms of reaction, the patient remains, notwithstanding the use of all proper remedies, in a very stupid condition; that he is with difficulty roused from his lethargy; and when questions are put, the expression or complaint is one of pain and uneasiness referred to the head generally. The eyes are intolerant of light; the skin is dry and hot; if the tongue can be protruded, it is covered with a white viscid secretion. Food is a matter of indifference, or is refused. The bowels have probably been copiously acted on by purgatives and injections. The pulse is in general, small, quick, and sharp. If there be wound or contusion, it should be examined and treated accordingly. But the symptoms now mentioned, as regards the brain and its membranes, are those of irritation and approaching inflammation. Without speculating upon the exact pathological condition of the contents of the cranium, which probably cannot be positively ascertained, these Mr Travers regards as cases, viewed as a class, in which quicksilver, administered so as to produce slight ptyalism, will effect the greatest good. Calomel, in doses of one or two grains, at intervals of three, four, or six hours, is the most eligible form; and if deglutition be difficult, or otherwise, the exhibition by the mouth is incommodious, the remedy should be smeared on the tongue. The mercurial ointment, which is also recommended, we must regard as a much safer, and not less effectual, mode of putting the system under the influence of the mineral. The gradual return of consciousness, the endurance of the light, the pulse becoming slow, soft, and full, the patient being less restless, often with copious discharges from the bowels, followed by desire for food, all shew the beneficial effects of the agent. After this crisis, seldom is it requisite to do more than watch, observe, and regulate the diet and other Non-Naturals.

The Observations upon Puncture of the Bladder are judicious. Mr Travers prefers puncture by the rectum, and expresses little apprehension as to the wound forming a fistulous opening. In point of fact, the urine speedily resumes it natural course. The operation is not very often requisite, and we are satisfied that it is still more frequently undertaken, than there is actual occasion for. It appears, however, that Mr Edward Cock has recourse to this operation upon more ordinary occasions than is customarily the rule. This gentleman employs it as a means of effecting a cure in certain cases of permanent impermeable stricture.

Mr Travers is not very favourable to the plan of treating strictures, by cutting down upon them from the perinæum. He is persuaded that, like the controversy about opening the sac in the operation for strangulated hernia, it will remain an open question.

We have serious doubts about the safety of making an opening into the interior of a joint when in a state of inflammation, or when distended by the morbid products of synovial inflammation. Although this proceeding has received the sanction of high authority, yet it has been so frequently, if not uniformly, followed by pernicious results, that it is impossible at present to view it in any other light, than merely a different mode of accelerating the fatal event. The reason of this, we think is not very obscure. But it would be unbecoming to speak with confidence. It is this. In general, if not always, the puncture has been made, while the inflammatory action was proceeding or only recently finished, and before attempts at adhesion and circumscription had been made. The result is that, in a large and extensively inflamed cavity inflammation of a disjunctive or erysipelatous character is developed. Let the termination of this acute state either be waited for, or let means to promote it be employed, by means of revellents, rest, compression by bandage, plaster and other applications. Then, if the inflammatory orgasm become circumscribed; if there be reason to believe that some limitation have taken place; it may be proper to make a small incision or puncture, because, at this time, and under these circumstances, there is less chance of the acute form of the disease being again kindled up within the joint.

The Observations contained in this volume possess great merit, and shew that Mr Travers is well qualified to sustain the reputation already acquired by that distinguised name.

PART III.

MEDICAL INTELLIGENCE.

I.—MATERIA MEDICA AND CHEMISTRY.

1. *On the Mineral Waters of Baden-Baden.* By SHERIDAN MUSPRATT, Ph.D., F.R.S.E. (Read before the Royal Society of Edinburgh.)

Baden is embosomed among hills, forming an offset of the Black Forest range, and seated on the banks of the Oos, an insignificant stream, which once, however, formed the boundary line between the Franks and Alemanni. It is famous for its beauty, but more so for its mineral springs, the most renowned in Germany, which were known to, and appreciated by the Romans, who fixed a colony in Baden, and called it *Civitas Aurelia Aquensis.* These springs are very much frequented, and their water is used internally and externally in all kinds of diseases, more especially for liver, mesenteric, and uterine complaints or obstructions. In fact, in 1845, nearly 40,000 persons resorted to them. The undoubted benefit derived from mineral waters in various diseases, attracted the attention of chemists from a very early date, to the composition of medicinal springs. A great number of analyses has been made of such waters, but most of them are imperfect, owing to the crude state of analytical chemistry when they were performed.

As yet, our speculations, regarding the part played by each constituent of mineral waters, are very vague, often in consequence of want of system in the arrangement of the various elements. So long as results are not tabulated according to some rule, it is impossible for the physician to institute a correct comparison, or to be able to judge upon which ingredient their effect mainly depends.

There are, in Baden, thirteen thermal springs, which gush out of the rock at the foot of the Schneckengarten. A fine temple-like structure is erected over the Ursprung, principal spring, one of the hottest and most copious sources. The massive vault which encloses the spring is of Roman construction.

It is strange that there is no English work wherein an analysis of the waters of Baden-Baden is given. [A.]

In order to have this deficiency remedied, Dr Sheridan Muspratt communicated to the Royal Society of Edinburgh, the following analysis, which may prove interesting to others further pursuing the subject.

Qualitative Analysis of the Water.—The water as it flows from the Ursprung, principal spring, has a temperature of 67·5° C.=153·5° F., the temperature of the surrounding air being 75° F.; it is clear and possesses a very faint animal odour, a trace of sulphide of hydrogen (?) and does not affect vegetable colour; it has a slight saline, or more properly speaking, a brothy taste.

The annexed experiments shewed the several ingredients of the mineral water. When the water was boiled for some time a small white crystalline precipitate subsided. The qualitative analysis was, therefore, divided into two parts.

1st, The analysis of the crystalline precipitate.
2d, The analysis of the substances in solution.

1st, The precipitate was treated with hydrochloric acid, which dissolved it all with effervescence, proving the presence of earthy carbonates and the absence of *sulphate of lime.* A portion of the hydrochloric solution treated with sulphocyanide of potassium, gave a faint red colouring, indicative of iron. The precipitate did not contain any magnesia.

2d, *Analysis of the Substances in Solution.*—A portion of the liquid filtered from the crystalline precipitate when evaporated to dryness did not effervesce with hydrochloric acid, proving the absence of alkaline carbonates.

A second portion of the liquid afforded on the addition of nitrate of baryta, a slight white precipitate, insoluble in hydrochloric acid, indicating sulphuric acid.

A third portion of the solution gave, with nitrate of silver, a copious white precipitate, easily soluble in ammonia, proving the presence of chlorides and the absence of iodides.

A fourth portion of the liquid was treated with hydrochloric acid, and evaporated to dryness over a naked lamp; on treating the residuary mass with water an insoluble part remained—silicic acid. A fifth portion of the liquid with the chloride of ammonium and oxalate of ammonia, gave a white precipitate of oxalate of lime. The filtrate from the oxalate of lime precipitate was divided into two portions; one gave slight indications of magnesia when treated with ammonia and phophate of soda; and the other portion when evaporated to dryness, yielded a residue, which, when treated before the blow-pipe, coloured the flame intensely yellow, and gave, after being heated to redness with an alcoholic solution of bichloride of platinum, indications of potassa.

By separating the carbonate of lime and evaporating the filtrate nearly to dryness, cubes of chloride of sodium appeared in company with silky crystals of sulphate of lime formed by double decomposition. If the liquid be evaporated to dryness, the residue on exposure readily deliquesces, shewing the presence of chloride of calcium. The substances present, and their respective amounts, in the imperial gallon, are the following:—

	Grains in the Imperial Gallon.	
Sulphuric acid	3.487	3·493
Chlorine	94·064	93·991
Silicic acid	2·947	
Carbonic acid in combination with calcia	6·240	
,, ,, protoxide of iron	0·514	
Sodium	52·152	
Potassium	7·182	
Calcium	4·000	
Calcia (lime) insoluble	7·943	
Magnesia	1·749	
Protoxide of iron	0·842	
Alumina, phosphate of lime, organic matter, &c.	Mere traces	
	181·120	
Residue given by direct experiment	181·129	

As the qualitative analysis proved the sulphuric acid not to be in combination with calcia (lime), the above bodies are hereafter tabulated in the exact state in which they exist in the water, thus:—

	Grains in the Imperial Gallon.
Chloride of sodium	132·644
Chloride of potassium	13·720
Chloride of calcium	11·040
Sulphate of magnesia	5·236
Carbonate of calcia (lime)	14·184
Silicic acid	2·947
Proto-carbonate of iron	1·356
Alumina, phosphate of lime, organic matter	Mere traces
	181·127
Specific gravity of the water	= 1·00284
Cubic inches of carbonic acid, per gallon	4·500

The only analysis I can find of the Baden-Baden water is by Kölreuter.* He gives the amount of each constituent in sixteen ounces of the water. I append his results calculated to the imperial gallon :—

Chloride of sodium	160·00
Chloride of magnesium	2·50
Chloride of calcium	17·50
Sulphate of lime	30·00
Carbonate of lime	16·60
Silica	3·30
Extractive matter	0·50
	230·40 (?)

His total amount of ingredients appears to be a great deal too much, moreover, he gives *sulphate of lime* as one of the constituents, and finds *no chloride of potassium.*

From the preceding analysis, it appears the principal ingredient is chloride of sodium. Hence it belongs to the class of saline waters.

It will be seen that Dr Muspratt, in the above analysis, or rather what may be called the analytic deduction, excludes sulphate of lime. For this he has fallen under the criticism of Mr James Grant of Bristol, who thinks that the reason assigned to justify the conclusion that the sulphuric acid is not in the Baden-Baden waters in combination with lime, is not satisfactory.† We confess that we think Dr Muspratt has arrived at the correct inference, namely, that the sulphuric acid is in union with magnesia rather than with lime, and that the lime is held in solution by excess of carbonic acid. There is indeed no reason why the sulphuric acid should not be in combination with sodium or potassium rather than with lime. But Dr Muspratt merely infers that it is not associated with lime, and that it is most probably associated with magnesia. Sulphate of lime is, correctly speaking, a product of reaction ; the expulsion of carbonic acid, and then union with lime, &c., decarbonated.

[A.]—This is a mistake. In the work of Mr Edwin Lee, The Baths of Germany, published first in 1839, and again in 1843, the following analysis of the Ursprung is given from Kastner.

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* Simon's *Heilquellen Europa's*, Berlin, 1839, p. 22.

† Pharmaceutical Journal, Nov. 7, 1851, p. 237.

	In one Pint.
Muriate of soda	17·500
Muriate of lime	1·500
Muriate of magnesia	0·500
Sulphate of lime	2·750
Carbonate of iron	0·111
	22·361

Carbonic acid gas half of a cubic inch.

It appears from this statement that Kastner commits the same sort of error as Kölreuter, although the presence of free carbonic acid might have enabled him to avoid this. The carbonic acid gas is manifestly employed in keeping the lime in solution; and this ought to have reduced the proportion of sulphate of lime.

2. *A Letter to Dr Lyon Playfair, being a Medical Commentary on the Results of his Analysis of the Buxton Tepid Water, &c.* By WILLIAM HENRY ROBERTSON, M.D., Senior Physician to the Buxton Bath Charity. London, 1852.

Dr Lyon Playfair analysed the water of the Thermal Springs of Buxton in the month of April 1852; and as the results, though not very widely differing from those of previous examinations, present, nevertheless, certain points of interest, they have induced Dr Robertson to publish some observations on the physiological uses and medical effects of these waters.

The water was collected partly in glass-stoppered bottles, and partly in earthenware jars. The gas as it issued from the crevices of the rock and bubbled through the water, was caught by an inverted funnel, and collected in glass bottles filled with the thermal water itself. These bottles were then sealed on the spot; and the evidence derived from the gas contained in them, shews that the precautions used for preventing the access of air were quite successful.

It is not necessary to describe the physical conditions under which the thermal springs appear at Buxton. It may be sufficient to state, that they issue from fissures in the limestone, and are accompanied by frequent, but intermittent bursts of gas, which escapes partly as large bubbles, and partly in innumerable small bubbles, giving to water freshly collected in glass vessels, all the appearance of soda water.

The water is clear, sparkling, inodorous, and when cool is almost tasteless. Its temperature is 82° Fahrenheit, and its specific gravity 1·0003.

Two points had specially to be attended to in the analysis of the waters,—firstly, to ascertain the nature and quantities of the ingredients in solution, and, secondly, the character and composition of the gas accompanying them.

In order to be sure that every ingredient came under his observation, Dr Playfair caused 100 gallons of the water to be evaporated down to about half a gallon, and examined the deposit and residual solution for bodies which might be present in such small quantity as to escape detection in the unconcentrated water. The precaution was found to have been necessary, for, in addition to the ordinary constituents of the waters, two more rarely occurring bodies, viz., fluorine and phosphoric acid, were found to be present, although only in minute quantity. The amount of fluorine was, however, sufficient to etch glass when applied with proper precautions. Neither iodine nor bromine could be detected.

The following analysis gives the amount and nature of the solid ingredients in one imperial gallon of the water at 60°:—

	Grains.
Silica,	0·666
Oxide of iron and alumina,	0·240
Carbonate of lime,	7·773
Sulphate of lime,	2·323
Carbonate of magnesia,	4·543
Chloride of magnesium,	0·114
Chloride of sodium,	2·420
Chloride of potassium,	2·500
Fluorine (as fluoride of calcium),	trace
Phosphoric acid (as phosphate of lime),	trace
	20·579

On examining the water there were found present carbonic acid and nitrogen, in addition to the solid ingredients. It was important to estimate the amount of the former in an exact manner. Some of the water was received from the spring into a glass-stoppered bottle, and the stopper was immediately inserted and secured. One gallon of the water was found to contain altogether 13·164 grains of carbonic acid; but of this quantity, 5·762 grains were due to the carbonates of lime and magnesia, and therefore only 7·402 grains could in any sense be considered as free. Again, the carbonates of lime and magnesia are present as bicarbonates, or as carbonates dissolved in carbonic acid, and 5·762 grains of carbonic acid would require to be added for this purpose. Hence, of the 7·402 grains, or 15·66 cubic inches of gaseous carbonic acid in the water, only 1·640 grain, or 3·47 cubic inches, can be considered as wholly free and uncombined.

The nitrogen in the water could only be present in solution, and not in combination; and as there is no very accurate method for ascertaining the precise quantity of this gas in the water at any given temperature, it was considered chiefly important to ascertain accurately the composition of the escaping gas, as this would indicate that of the gas held in solution. The following are the analyses of two portions of the gas collected as formerly described, the analyses being given *according to volume.*

	I.	II.	Mean.
Carbonic acid,	1·169	1·164	1·167
Nitrogen,	98·831	98·836	98·833
Oxygen,	trace	trace	trace
	100·000	100·000	100·000

The gas, therefore, consists entirely of carbonic acid and nitrogen; for the oxygen, which did not amount to one-tenth per cent., may be viewed as quite accidental, arising probably from the corks used to close the bottles.

Judging from the analysis and proportion of the gases, it is assumed that, *at the moment of issue*, the water is charged with 206 cubic inches of nitrogen, and 15·66 cubic inches of carbonic acid. This assumption is founded upon the proportional relation of the two gases. The proportion of carbonic acid in the water being determined, and the proportion of carbonic acid to that of nitrogen contained in the water being 1·2 to 98·8, the amount of nitrogen contained in the water at the moment of issue may fairly be assumed to be 206 cubic inches per gallon.

Before remarking farther on the above analysis, it may be useful to refer to that by Scudamore. The analysis given by him was upon the wine gallon, which is one-fourth less than the imperial gallon. Correcting for this difference, Scudamore found 20 grains of solid matter in a gallon, a result not materially different from that detailed above. The solid ingredients do indeed differ to some extent in the two analyses; but it must be recollected that analytical chemistry is now in a much more advanced state, and, instead of being surprised at the differences, we are rather inclined to admire the precision with which the points had been made out.

"From a consideration of the previous analysis," says Dr Playfair, "I am inclined to ascribe the medicinal effects of the water almost entirely to its gaseous constituents. The water, deprived of its gases, has the composition of an ordinary spring water, with the exception of the fluorine and phosphoric acid, both of which are present in mere traces; and it is therefore difficult to conceive that they can have any medicinal effect when the water is used for baths. The gases are, however, nearly of the same composition as those of the thermal spring at Bath, and there is no reason to doubt that dissolved carbonic acid and nitrogen may exert important physiological effects. At all events, the singular chemical character of the Buxton tepid water must be ascribed to its gaseous, and not to its solid ingredients."

It appears that, as had been always known, the most abundant ingredient is carbonate of lime, which is manifestly kept in solution by the carbonic acid; then carbonate of magnesia, held in solution also by excess of carbonic acid; then chloride of potassium, chloride of sodium and sulphate of lime in the order specified, but all three nearly in the same amount; then oxide of iron and alumina, and chloride of magnesium.

One substance only now shewn to be present is fluorine, in the form of fluoride of calcium. But in December 1846, when speaking of Dr Wilson's discovery of fluorine in the water of the wells of the Cowgate of Edinburgh and various other waters, we stated our belief that fluorine exists in the Buxton waters, and in the mineral waters of Derbyshire in general.* This belief was founded on two circumstances; one, the geological nature of the district, containing much fluate of lime; the other, the fact that glass bottles, much used at Buxton to contain the Buxton water, were, in general roughened and corroded. These waters are now shewn, by direct experiments, to contain fluorine, though in small amount.

Phosphoric acid is also shewn to be present in union with lime.

Dr Lyon Playfair and Dr Robertson are not exactly agreed as to the efficient agents in the Buxton water. Dr Playfair speaks with the confidence of the chemist, that the singular chemical character of the Buxton tepid springs are to be ascribed to the gaseous agents, namely, nitrogen and carbonic acid; not to the saline ingredients; and he is inclined to ascribe almost entirely to the same articles the medicinal effects of the water. Dr Robertson, on the other hand, while he admits the great influence of the gaseous agents, and especially the nitrogen, in producing beneficial effects on the human body, thinks, however, that these effects may yet have to be ascribed to the presence of some hitherto undetected agent. This is by no means improbable. Indeed, when we remember how many agents previously unknown to exist in mineral waters, have within the last ten or twelve years been shewn to be contained in them, it seems almost impossible to say, what are the active substances upon the operation of which the effects of mineral waters may depend. Thus, iodine, bromine, fluorine, silica, arsenic, crenic acid, and apocrenic acid, have all been found to exist in various mineral waters, in which their presence had not been previously suspected.

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* Edinburgh Medical and Surgical Journal, volume sixty-seventh, p. 309, Jan. 1847.

To us, further, it appears, that there are not only no grounds for adopting the opinion of Dr Playfair, that the saline and metallic ingredients are inert, and that the chemical character of the Buxton tepid water must be ascribed to its gaseous and not its solid (saline and metallic) ingredients; but good grounds for thinking that these substances must have considerable effect. We confess, indeed, that the latter expression is to us unintelligible. How water, containing lime, magnesia, chloride of sodium and potassium, with silica and oxide of iron, is to be regarded as not having thereby its chemical characters influenced, is a matter which we cannot understand. It is true these articles are in no great proportion; but why should it be inferred that, therefore, they have no influence. The Buxton water belongs, indeed, to the head of what the German chemists call chemically indifferent springs; an expression which, to speak as gently of it as possible, conveys no meaning. Because physicians are in the habit of giving saline and metallic substances in large doses, they consequently infer that when nature gives them in minute doses, therefore these doses are inert. Do not certain substances so ingested accumulate in the system? If they do so, are they void of all influence? But whether they are accumulated or not, we have no reason to believe that they are entirely without effect upon the organism.

The ascription of great, if not peculiar, effects to the presence and operation of nitrogen gas must be admitted to be reasonable enough. This gas has been long known to exist in greater or less amount in not a few mineral waters; and probably it is an ingredient next in frequency to hydro-sulphurous acid gas and carbonic acid gas. Priestley first in this country discovered nitrogen in the Bath mineral waters in the year 1775.* Dr George Pearson discovered the same gas in the Buxton waters in 1782;† a fact which was confirmed by Sir Charles Scudamore and Mr Garden, in the year 1819, with the additional information that the amount was about one-fifth more in the gallon than had been found by Dr Pearson.‡ The Bath waters all subsequent analyses have shewn to contain a considerable amount of nitrogen. Scudamore also shewed its existence to the amount of 1·19 per cent., or 2¾ cubic inches in the gallon, in the Tunbridge Wells water; and in that of Harrogate to about 1·25 = 5·8 cubic inches in one gallon. Nitrogen was also found by Dr Daubeny in the thermal waters of Bakewell and Stony Middleton in Derbyshire, and that of Taafe's well in South Wales.§ The sulphurous spring of Holbeck near Leeds contains it to the amount of 1·64.‖

Near Inverkeithing, in the county of Fifeshire, in Scotland, is a cold spring emitting nitrogen; and Sir Roger Murchison mentions one of the same kind in Shropshire.

On the Continent of Europe, nitrogen has been found to form an ingredient of many mineral springs. Hermbstadt found the sulphurous waters of Doberan to contain 28·16 per cent., mixed with carburetted hydrogen; and those of Nenndorf 20·61 per cent. The Porla acidulous spring in the borders of the Provinces of Nerikia and Westmannland in Sweden, discharges copious bells of gas, which was found by Berzelius to consist of seven parts of azote, one part of carbonic acid, and a minute quantity of carburetted hydrogen. The acidulous springs of Nocera in the Papal Dominions, contain, according to Morichini, 5·84 of gaseous matters, of which 3·26 are nitrogen, the residue being oxygen and carbonic acid, in the proportion of 1·83 and 0·75. Nitrogen is found in greater or less amount in the thermal waters of Aachen in Rhenish Prussia, Baden in Austria, Wiesbaden 0·31, Ems 0·21, Schlangenbad 0·069, Wildbad in the Black Forest, Karlsbad, Teplitz, and Gastein.

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* Experiments and Observations on Air. By Joseph Priestley, LL.D., F.R.S. The Second Edition. London, 1775, vol. ii., 225.

† Observations and Experiments for Investigating the Chemical History of the Tepid Springs of Buxton. In Two Volumes. By George Pearson, M.D. London, 1784, vol. ii., pp. 15, 21, 43, and 210.

‡ A Treatise on the Composition and Medical Properties of the Mineral Waters, &c. By Sir Charles Scudamore, M.D., F.R.S., &c. Second Edition, London, 1833.

§ Daubeny on Volcanoes, p. 558, chapter xxxv. Second Edition, London, 1848.

‖ Philosophical Magazine, vol. i., 248.

The cold springs near Kissingen emit copious bells of gas which Kastner found to contain in the 100 parts 25·45 of carbonic acid, 66·5 of nitrogen, and 8·05 of oxygen.

In France nitrogen has been found in the waters of Neris, Bareges, Cauterets, St Sauveur, Aix, Bourbonne les Bains, Luxeuil, Balaruc, Avene, Mont d'Or, Chaudes Aigues, Vichy, and at Leuk and Pfeffers in Switzerland. From many of the hot springs on the Lower Alps, it is evolved in a state nearly quite pure, as at St Gervais, Courmayer, St Didier, Bonneval. In the last of these alone is it associated with any carbonic acid, namely about 12 per cent.

In Italy, nitrogen is found in the spring of Castellamare in the Bay of Naples; at Monte Catini, near Pisa, in Tuscany, and in the hot spring of Torre del Annonziata, mixed with a large excess of carbonic acid.

This gas is found also in the cold springs of Eilsen, Pyrmont, Gleissen, and it is found in the springs of Monghyr in India, in those of Onoto and Mariana in South America.

Dr John Davy discovered nitrogen gas almost pure issuing in large quantities from a hot spring near Trinkomalee in Ceylon.

It is found in small quantity in the Congress spring of Saratoga; and in the mineral waters of Rensselaer in the State of New York, it is discharged in copious bells from the gravelly bottom of the basin.

From this list, which might be considerably enlarged, it appears that nitrogen is in different amounts, if not a very general, certainly not a rare article in mineral waters. In many cold springs, in which a minute quantity only is present with carbonic acid and oxygen, it is supposed to be derived from the atmosphere, by the absorption of the oxygen. But when it appears, and is evolved in large quantity, as at Steinbad in Teplitz, Wildbad in the Black Forest, Buxton in Derbyshire, and Bath in Somersetshire, it is certainly to be viewed as an independent constituent of mineral waters, whatever may be the source from which it is derived. Bischoff has thrown out the idea, that meteoric water, flowing into vast subterraneous caverns may there meet a stratum of air, which being compressed above the water will by it be absorbed. Should the water in that case descend to such a depth that its temperature is increased, it will readily part with the oxygen of the atmospheric air, retaining, however, the nitrogen and the carbonic acid gas; and in this manner he thinks a mineral water may contain the latter two gases without the former.* This, however, it may be remarked is mere conjecture.

The thermal spring of Wildbad in the Black Forest contains, with a minute proportion of saline matters, a large amount of nitrogen, 80 per cent.; and to the presence of this gas, the German physicians and Dr Sutro ascribe the curative effects which the use of the Wildbad water exerts upon chronic rheumatism, rheumatic gout, and stiffness and nodosity of the joints.

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* Poggendorf's Annalen der Physik und Chemie. Band xxxii., theite 241.

So also the waters of Pfeffers in the Canton of St Gallen in Switzerland, and that of Gastein in the Mountains of Salzburg, contain, the former a small proportion of nitrogen, the latter a good deal more (2·02 in 100 parts of water). It seems therefore quite natural to ascribe to the presence of this gas very notable effects upon the human organism; though in what exact manner these effects are produced, it is not so easy to understand and explain.

The opinion of Dr Sutro is given in the following passage:—I should not go so far as to attribute a nourishing property to the azote introduced into the absorbent vessels with the highly diluted water. But when it is admitted, on all hands, that our tissues constantly discharge wasted particles in proportion to the regular additions provided by the arterial supplies, and when we know a great part of this waste to issue from out cutaneous pores in a gaseous form, would it not be reasonable to attribute some restoratory function to the contact and combination of the gas with organic particles? We know that, in old age, earthy or inorganic formations prevail in the reproductive sphere. Limbs become more rigid, the joints less pliable, secretions retarded, execretions diminished, vital elasticity and resisting power impaired. Substances ordinarily carried rapidly along the vascular canals in a dissolved state, are now precipitated out of the slowly moving mass, and deposited in spaces where they further impede voluntary movement.

If we see the use of a mineral water, causing distinct retrogression of these anti-vital phenomena; if we perceive gouty concretions to proceed towards absorption; if we observe contracted limbs gradually to relax again, and to try feeble efforts of long-forgotten exercise; if we find cutaneous harshness and rigidity to diminish, and to give way to a former softness; if we behold a resuscitated desire for muscular exertion and for mental work in a prostrate individual, and we know the spa, the originator of these changes, to possess a great quantity of azote, is it not legitimate to attribute to this gas part of the efficacy?

The opinion of Dr Robertson is contained in the following passage. "If the free application of uncombined nitrogen to the surface of the body, or to the lining membrane of the stomach, is capable of influencing the system in any important degree, the medicinal effect of the tepid water of Buxton must be admitted to be so far explained. Whatever might be the effect of the gaseous constituents of the water upon the system, it might be inferred that such effect would be rapidly developed; and any difference of effect between the use of this water, and that of common spring water at the same temperature, which may be experienced at the time of bathing in it or immediately afterwards, or at the time of drinking it or immediately afterwards, may perhaps be referable to the large amount of free nitrogen which it has now been shewn to contain. With this better information as to the amount of the constituent gases, it need no longer be matter of surprise that the internal use of the water is so often attended with some degree of dizziness immediately after drinking it, or even that, in much rarer cases, its immediate effect is to stimulate to the extent of intoxication; and it need be no longer matter of surprise that the use of the bath of this water is usually followed by so remarkably perfect a degree of reaction; or that, whether the water is externally or internally used, the appetite and digestion should be so generally and to so great a degree promoted, and the vascular and nervous systems so appreciably stimulated. These effects, in whatever degree they may be presented, may probably be referred to the influence of so large an amount of nitrogen, thus offered to the system in a free and readily available condition."

The Buxton water exerts in some persons a secondary effect, more lasting on the system. This consists in a certain amount of febrile disorder, followed by languor and debility, which is manifested twelve, eighteen, or twenty-four hours after the use of the bath, and in some instances follows the internal use of the water without the use of the bath. This Dr Robertson regards as a water fever. It does not, however, correspond exactly with the secondary or water fever observed to follow the use of some of the German mineral waters, which is in general not a daily fever, but a state of disorder and illness arising after the use of the waters and the baths have been continued for sixteen or twenty days, and which is evidently the effect of accumulation, and consequent irritation in the system from the presence of an excessive amount of mineral articles.

This secondary or febrile effect of the Buxton waters Dr Robertson regards as probably depending on the presence of some agent not hitherto discovered; and with this undiscovered agent, he thinks are connected the action and utility of the Buxton water as a curative agent. It is certainly not easy to say what substances further and more improved analysis, and more powerful analytical agents, may bring to light; and hence it is not unreasonable to think, that to some new substance the effects of the Buxton waters may in part be traced. We feel it necessary to say in part; because, with the greatest respect for the opinions and experience of Dr Robertson, we think that when the Buxton water is compared with several other mineral waters to which efficient results are ascribed, both in this country and on the Continent, we find it not behind or inferior to them in its chemical constituents. Take, for instance, the Malvern Springs. Though they are very pure and little impregnated with mineralising articles, yet they produce very decided effects, and are remarkable for producing the same stupefying and confusing feelings in the head which the use of the Buxton waters does. Indeed so uniform and considerable are these waters in causing vertigo, drowsiness, and painful effects in the head, that we think it not improbable that they contain nitrogen as well as the Buxton waters.

We must not however dwell longer on this subject. All that we are desirous to establish is the inference, that there is no good reason to suppose that, however small, however insignificant, in human and medical eyes, may be the quantity of saline and metallic ingredients in any mineral water, that mineral water is to exercise little or no effect upon the animal body and its functions. We know absolutely nothing of the amount of substances required to produce decided effects; and certainly nothing, where many different substances are combined and dissolved in a large quantity of water.

The conclusion of the whole matter after all, becomes entirely a question of experience and observation of actual results. Dr Playfair ascribes the effects of the Buxton waters, principally to the presence and operation of nitrogen, and thinks that the saline ingredients may be thrown aside. Dr Robertson thinks that all the medicinal effects of the Buxton tepid water, and especially its great alterative action, can scarcely be ascribed to the large amount of nitrogen, but may be owing to the presence of some ingredient not hitherto discovered. He adds that the great and lasting effects of these baths, and the internal use of the water, are unquestioned and indisputable. Their use is almost specific for the relief or alleviation of most cases of rheumatism, and of many cases of gout, for which the use of other means and appliances has been sought and tried in vain. In proof of this, he adduces the fact that large numbers of poor handicraftsmen, who have proved the effect of this water on their suffering and imperfectly usable limbs, are known to undergo great privations, in order to secure its use at stated intervals, from finding that no other means within their reach are capable of maintaining such a state of their joints as is needful to enable them to follow their employment. The yearly reports of the Buxton Bath Charity certify, that of 15,497 patients, for the most part sufferers from rheumatism, admitted to the benefit of the institution, from the year 1838 to 1851, only 613 had to be sent home as being "no better," the large proportion of 11,740 having been discharged as "cured or much relieved."

II.—MEDICAL PATHOLOGY AND THERAPEUTICS.

3. *Instance of Wound of the Stomach with Protrusion of the Organ, terminating favourably.* By W. Ashby, M.D., of Culpeper, C.H., U.S. (Medical Examiner and Record of Medical Science. April 1852, p. 271).

It is not easy to say whether the following report is real or a sort of jest. We give the report as we find it; and readers will deduce their own inferences.

A negro boy, aged six years, the property of Mr R. B., fell, whilst running down a hill, upon a pair of sheep-shears, which he had in his hand. The instrument penetrated the stomach obliquely from above, just grazing the left side of the sternum and edges of the ribs, making a flaplike orifice in the integuments. Dr Ashby was called in consultation by Dr P. C. Slaughter, and found nearly the whole stomach protruded, and discharging its contents through an aperture about three-quarters of an inch in length. Aware of the controversy which has so long existed among able surgeons, on either side, as to the propriety of stitching the stomach or bowels, the everted edges and gaping appearance of the wound in the stomach made it necessary, Dr Ashby thought, that a stitch should be taken. To avoid irritation as much as possible, with the finest needle and silk he carried a single stitch through the middle of the wound. Before he saw the case, Dr S. had made some efforts to restore the organ to its natural position, but it did not occur to Dr Ashby at the time that he should have any serious difficulty in replacing it, at least after enlarging the orifice a little.

So restless, however, was the boy, so violent his screaming and resistance, the nausea and vomiting which constantly attended the handling of the stomach, that notwithstanding Dr Ashby enlarged the orifice several times to a considerable extent, the best efforts not only failed to restore the organ, but it seemed to protrude the more. At this juncture, fearing the irritation resulting from further efforts, Dr Ashby suggested the use of chloroform, notwithstanding the necessary delay of having to send several miles for it. Whilst under its influence, Dr Ashby found it necessary again to enlarge the aperture slightly, and then had no farther difficulty, although the boy vomited as freely as before from handling the organ. The wound of the integument was rather ragged in its appearance, and of course a little bruised by the efforts. The wound of the stomach was brought directly opposite the tegumentary wound, and gently retained within its verge. A single stitch, patent lint, with cold water and a bandage, completed the dressing. The patient was placed on his side, absolute rest enjoined, and soon afterwards a large dose of opium was administered. From the time of the accident until the completion of the dressing six hours intervened, and yet the boy retained his strength most remarkably. Under the influence of the opium the patient rested well the first night.

2d day.—This morning the pulse is a little excited, and face flushed. V. S. to make a decided impression; and this was repeated twice during the day, and opium after each bleeding. Low diet enjoined; but the boy desires no food.

3d.—The wound had a healthy appearance, but tenderness of the abdomen and tympanitis greatly increased the fears of the attendants as to the result. The pulse feeble and quick; the bowels not moved since the accident. Turpentine enema and a succession of blisters were ordered, and after the bowels were moved the opium was resumed.

4th.—The patient evidently improved. Tympanitis and tenderness diminished; pulse more quiet; countenance and general aspect of things more encouraging. He takes a little hot water tea this morning for the first time; gum water and opium continued.

5th.—The wound not healed by the first intention; has a dark spot immediately over the wound of the stomach, and is discharging a very offensive sanious matter. A soft poultice, and the same prescription continued.

6th.—The ligature came out this morning. The same prescription continued. From this date the boy gradually recovered, without any particular change in the treatment.

Remarks.—1st, It has occurred to Dr Ashby, that possibly it would have been better to have restored the stomach, at least partially, before the stitch was inserted, as he incurred the risk of breaking out the ligature by the subsequent efforts at reduction; and he thinks that the accumulation of gas, though some escaped with an audible sound several times, did not increase the difficulty.

2d, This case was well adapted to the use of chloroform, and illustrates more happily its incalculable value when used with discrimination.

3d, It is important here to impress the principle, that an *inflamed or diseased organ must have rest.* In this case the stomach, instinctively sensible of its wounded and disabled condition, refused most emphatically, for four days, to receive any nourishment,—not even gum water,—and but very little of anything for about ten days, notwithstanding the entreaties of master and friends contrary to our orders.

This principle of rest is susceptible of very extensive application in practice; and any inflammation can be cured, he believes, to which it can be applied. In conformity to this important law of the animal economy, in the above case, Dr Ashby gave opium freely, to prevent nervous and vascular reaction; and by thus aiding in keeping the wounded organ in a state of repose, it contributed, it is believed, no little to the favourable result.—*Stethoscope (Virginia), United States.*

4. *Maguey, or Agave Americana; a Remedy for Scurvy.* By Glover Perin, M.D., Assistant Surgeon, United States Army. (New York Journal of Medicine. Sept. 1851. Vol. viii., No. 2.)

While a division of the United States Army was stationed at Fort MacIntosh, Laredo, Texas, in May 1851, scurvy made its appearance among the soldiers, and speedily rendered many unfit for duty. Lime-juice was given as usual; but it could be procured only in limited quantity; and Dr Perin naturally was led to inquire, whether the citizens of the town of Laredo, who are alike destitute of vegetables, had not been affected by this disease; and, if so, what domestic remedies they employed.

Dr Perin was informed by the curate of the town, an intelligent Catholic priest, that a few cases had come to his knowledge, and that he once suffered from an attack of scorbutus in his own person. He gave an account of his own case, and of the domestic remedies resorted to.

He stated further, that he left his bed on the 9th day from the time he commenced the use of his remedies, and that by the 15th he was so far recovered as to resume his parochial duties. He did not know to what agent he should attribute his rapid recovery, but expressed his belief in the superior efficacy of warm bathing, which he used daily.

Among the domestic remedies which he used, and the one which appeared to be the chief, was the *Maguey*, or *Agave Americana.* This plant is mentioned among the unofficinal articles in the United States Dispensatory as the American Aloe, and " is said to be laxative, diuretic, and emmenagogue." As officinally described, it is the Agave Americana; *Nat. Order* Bromeliaceæ; *Sex. Syst.*, Hexandria Monogynia.

As the Maguey appeared to be the remedial agent in the curate's case, Dr Perin determined to make a trial of its effects upon some of the patients suffering from the scurvy in this command.

An infantry soldier admitted into hospital on the 25th March 1851 with distinct and severe symptoms of scurvy, was the first tried with the Maguey. He had been placed upon lime-juice, diluted and sweetened, with good diet. Vegetables could not be procured.

On the 11th April he was placed upon the expressed juice of the Maguey; on the 17th he was strong and convalescent, the purple spots having disappeared. On the 7th May he was almost well.

In another instance the juice of the Maguey was equally beneficial.

Eventually in eleven cases, in which no improvement appeared to be taking place under the use of the lime-juice, continued for many days; and in another set of seven cases, all of which up to 21st April 1851, had made no progress, the Maguey juice was exhibited. Speedily the men all begun to get rid of their symptoms, and the improvement is stated, in general terms, to have been rapid.

The countenance, so universally dejected and despairing in the patients affected with scurvy, is brightened up by contentment and hope in two days from the time of its introduction; the most marked evidences of improvement were observable at every successive visit. From observing the effects of the Maguey in the cases which have occurred in this command, he feels compelled to place it far above that remedy which, till now, has stood above every other;—the lime-juice.

This no doubt will appear strong language; but Dr Perin expects that it will be verified by further experience.

The juice of the Maguey contains a large amount of vegetable and saccharine matter, and of itself is sufficiently nutritious to sustain a patient for days.

This succulent plant grows indigenous in most parts of the State, and, if Dr Perin be correctly informed, in New Mexico and California. In Mexico it is well known as the plant from which they manufacture their favourite drink, the Pulque, and grows in great abundance. As it delights in a dry sandy soil, it can be cultivated where nothing but the cactus will grow; for this reason it will be found invaluable to the army at many of the western posts, where vegetables cannot be procured.

The manner in which it is used is as follows. The leaves are cut off close to the root; they are placed in hot ashes until thoroughly cooked, when they are removed, and the juice expressed from them. The expressed juice is then strained, and may be used thus, or may be sweetened. It may be given in doses of two ounces to three ounces three times daily.

It is not disagreeable to take, and in every instance it has proved to agree well with the stomach and bowels.

After the leaves have been cooked, the cortical portion near the root may be removed, and the white internal portion may be eaten; it appears to be a wholesome and nutritious food. The muleteers use it in this way, and they seem to be very fond of it. Dr Perin has been informed, upon good authority, that several tribes of Indians in New Mexico make use of it in the same manner. The use of the leaf in this way, he believes will ward off most effectually incipient scurvy.

A great benefit, Dr Perin feels confident, will accrue to the army by the introduction of the Maguey as a remedial agent in the treatment of scurvy.

5. *The Treatment of Secondary, Constitutional, and Confirmed Syphilis by a Safe and Successful Method; with Numerous Cases and Clinical Observations, illustrating its Efficiency and Mode of Application, in the more obstinate and complicated Forms of the Disease.* By LANGSTON PARKER, Surgeon to the Queen's Hospital, and Professor of Anatomy and Physiology in Queen's College, Birmingham, &c., &c. London, 1850, 12mo, pp. 112.

Mr Langston Parker believes that it is not possible to cure the great majority of Venereal Diseases without mercury. He has, however, like others, seen many patients who could not, without mischief, take even a few grains of quicksilver internally, and who could not use it even in friction. Like others, also, he has seen patients broken down by mercurial courses, with their disorders not only uncured, but actually made worse than when they commenced the use of medicines.

For these reasons the author thinks it proper to give full details on a method of curing the symptoms and effects of syphilis by means of quicksilver administered in a manner which he represents to be safe and successful; that is, free from the evils of the ordinary mode of administration, and effectual in accomplishing a cure.

The method cannot be said to be new; but it is probably one, in which the mineral is applied in a more methodical and systematic method than heretofore; and under all circumstances it is right that the details of the method should be made known. It consists essentially in applying quicksilver to the system in the form of vapour by means of fumigation.

Mr Langston Parker denominates the method which he employs, the Mercurial Vapour-Bath, because it consists in surrounding the patient with an atmosphere of mercurial vapour in a moist state. It has all the advantages attributed to the best conducted, most successful, and least hurtful mercurial treatments conducted in the ordinary modes. It has none of their disadvantages. It neither produces mercurial fever, salivation, ulcerations of the mouth, erethismus, debility, or any form of cachexia, so common from prolonged ordinary mercurial treatments. It has the superior advantages of effecting speedy and permanent cures, without confinement and without risk.

The patient is placed on a chair, and covered with an oilcloth lined with flannel, which is supported by a proper framework. Under the chair are placed a copper bath containing water, and a metal plate on which is put from one to three drachms of the bisulphuret of mercury, or the same quantity of the grey oxide, or the binoxide. Under each of these a spirit-lamp. The patient is thus exposed to the influence of three agents, heated air, common steam, and the vapour of mercury, which is thus applied to the whole surface of the body in a moist state. After the patient has remained in the bath from five to ten minutes perspiration generally commences, and by the end of twenty or thirty minutes, beyond which the bath is not prolonged, it is generally excessive. The lamps are now removed, and the temperature gradually allowed to sink; when the patient has become moderately cool the coverings are removed, and the body rubbed dry; the patient is suffered to repose in an arm-chair for a short time, during which he drinks a cup of warm decoction of guaiacum, sweetened with syrup of sarsaparilla.

The apparatus requires some modification and management to suit particular cases. Where it is wanted to induce a quick and decided action the whole power of the bath should be brought into operation, and the largest quantity of mercury should be employed. In rapidly-spreading ulcers this is required. Again, in chronic skin or throat diseases, where a powerful action would rather oppress the patient than cure his disease, the power of the bath should be modified, and not so great a heat or so much mercury employed. This is accomplished by using smaller spirit-lamps, or, when perspiration has once been induced, by the removal of one lamp, leaving the patient thus exposed for a time to the mercurial vapour alone. This should be done where the patient has been broken down by long-continued disease, in bad or weak subjects, or where a more prolonged action is required to eradicate the more deep-seated effects of the venereal poison, as in diseases of the bones, or indurations on the penis. Each particular case would require a greater or less modification of this kind. The form of mercurial employed is also of consequence. In skin diseases the bisulphuret is to be preferred; in diseases of the throat or nose the grey oxide, or binoxide is better, because the patient can bear the head immersed without sneezing or coughing, which he cannot do when the bisulphuret is used.

Mr L. Parker is in the habit of using four mercurial preparations for the bath; the bisulphuret of mercury, the binoxide of mercury, the grey or black oxide, and the iodide. These may be used singly or combined in different ways, to suit the peculiarities or emergencies of each particular case. The first three preparations are milder than the last, and from half a drachm to four drachms may be used for a bath with perfect safety. In one case half an ounce was used for each bath, and two applications were sufficient to bring the system fully under the influence of the remedy. The iodide must be used in smaller quantities; nearly the whole of this preparation is rapidly converted into vapour, and, unlike all the other preparations, leaves scarcely any ash behind it. From five grains to half a drachm of the iodide is sufficient, and it is better to use it in small quantities, mixed with a larger quantity of either of the other preparations. In affections of the testes (sarcocele) and of the bones (the various forms of ostitis, or periostitis) a combination of a scruple of the iodide, and one or two drachms of the bisulphuret or binoxide would be a proper form. For local application to the cavities of the nose or mouth, a few grains only should be employed, as the vapour of the iodide of mercury is more irritating and more powerful than that of either of the other preparations mentioned.

A short preparatory treatment should be adopted before using the baths. The bowels should be kept free, and the use of wine, spirits, &c., prohibited. The patient should be free from fever, the tongue clean, and the freedom from organic diseases, such as those of the heart and lungs, more particularly, should be ascertained. Should such, or other complications be present, they might require modifications of treatment, but would not prevent its employment, as this is not only the most certain, but the safest way of curing all forms of constitutional syphilis.

The plan of treatment now recommended requires not that the patient should forego his ordinary occupations of business, or that he should be confined to the house during its use. It must be admitted that its effects would be accelerated by confinement to bed, or to a couch in a moderately warm room; but this is by no means necessary; Mr Parker has rarely advised it, except in such cases where exposure or exercise would be positively mischievous, as in cases of sloughing, or rapidly-spreading ulcers in the throat or elsewhere.

The diet should be light, nutritious, and unstimulating; milk, chocolate or cocoa, night and morning; animal food for dinner, with weak wine and water. Where the patient has been reduced by mercury given internally, or by a combination of syphilis and mercury, the diet may be more nutritious; but stimulants should be avoided. Smoking must be prohibited, particularly in diseases of the throat and nose.

In a great majority of cases the moist mercurial vapour, employed as here directed, is capable of curing the disease without the assistance of internal medicine; but the cure is generally expedited by the administration of the latter in small quantities. The treatment is always assisted by the decoction of sarsaparilla or guaiacum, drank warm night and morning, and immediately after leaving the bath. Mr L. Parker prefers the latter, the compound decoction, made according to the formula of the Edinburgh Pharmacopœia. Where other medicines are required to assist the treatment, and particularly the various preparations of mercury, a small quantity is required when the patient is using the vapour. Several instances in which diseases have obstinately resisted large quantities of mercury, given for long periods, yield immediately when the baths were employed. The effects of mercury upon the system become very quickly manifest under the influence of the baths, when the system had previously resisted this influence. When Mr Parker employs mercury internally, during the use of the baths, it is either under the former of the biniodide, or bichloride given in solution in small quantities, not exceeding the twentieth of a grain for a dose. The use of this medicine in drachm doses of the ointment in form of friction, in five grains of blue-pill or calomel, two or three times a day, under the old plan of treating venereal diseases by mercury, can never be required, unless it is wished to break up the health and constitution of the patient. Many persons have never recovered from internal mercurial treatments of this kind. He never saw the most delicate patient, either male or female, whose health was injured for one hour under the plan now recommended, and he had very rarely seen a disease that has not been cured without a relapse. The experience derived from the treatment of many hundred cases warrants him in speaking thus positively on the subject.

The time occupied in the cure of venereal diseases by the mercurial vapour-bath is much shorter than that consumed by any other kind of treatment; its effects are commonly immediate, one full bath very frequently making at once an impression on the disease. Where the hair has been falling rapidly, one bath has arrested this; ulcers which have been rapidly spreading have been rendered stationary by one bath. After two or three baths, the improvement is in most instances marked; and the cure is effected in one-fourth, or even one-sixth of the time required for the success of ordinary treatments. The nature of the cases determines the time occupied in the cure. In superficial skin diseases, or superficial ulcers of the nose and throat, the cure is very rapid. Mr Parker has constantly known affections of this kind entirely cured in a fortnight or three weeks, without fear of relapse, and with pleasure rather than inconvenience to the patients.

In enlargement of the bones and testes, in indurations of the penis, persistent induration of the cicatrix of a primary sore, the cure is necessarily more tedious; the change of structure produced in such diseases must have time for removal. In these cases, nevertheless, which require months of treatment under common circumstances, and which are not unfrequently considered or given up as incurable, the moist mercurial vapour will do more in one month than any other treatment in six. He states cases of induration of the penis that have been removed in three or four weeks, which have not shewn the slightest disposition to amendment after weeks, and even months, of ordinary internal treatment.

All authors and all surgeons conversant with the treatment of syphilitic diseases, admit the frequency of relapses under ordinary treatments; hence constitutional diseases are to be feared as the result of primary ulcers; and when one form of constitutional taint has been apparently cured, it is commonly, after a time, succeeded by another. Thus venereal diseases run through those phases or stages which have been termed primary, secondary, and tertiary; and it has been a primary object in all plans of treatment to have recourse to those which, while they cure the disease then present in the best manner, and with the least risk, shall prevent the occurrence of future diseases under another form. Nothing is more uncommon than a relapse after treatment by the vapour. Mr L. Parker recollects not more than three or four occurrences of this kind, and these have been of the most trivial character. He has generally given the patient the most positive assurances on this point, and he has not been disappointed. He has not seen secondary symptoms succeed to primary sores treated by the baths, which have been used in all cases that would probably be followed by such an occurrence.

The effects of the mercurial vapour-bath upon the patient vary under different circumstances. If the general health of the patient be apparently good, and we have to control a single isolated symptom of disease, such as a primary sore, an enlarged testis, or an indurated cicatrix, and the baths be used too frequently, the patient would become a little languid, and probably a little thinner; this would be avoided by properly timing the intervals between the baths. Should the patient be labouring under general constitutional taint, and exhibit as local symptoms, loss of hair, sore throat, ulcers of the nose, or skin diseases, he almost invariably gets fat under the treatment. The mouth is commonly affected, after using four or six baths, more quickly if the head be immersed, which is better; the patient can bear the head in the bath for five, ten, or even twenty minutes without inconvenience; patients vary in this particular; and it depends very much on the form of mercurial preparation employed. The gums, when affected, are red, elevated, and tender, but the baths never produce salivation, or ulceration of the mouth.

Some forms of constitutional syphilitic diseases more readily yield to the use of the vapour than others. Some are cured with an extraordinary degree of rapidity, and are perfectly cured, which is proved by their not having relapsed, or presented a fresh venereal symptom after many years. These forms are superficial diseases of the skin, loss of hair, superficial ulcerations of the nose and throat.

Some varieties require a longer treatment, as diseases of the deeper-seated parts of the skin, some forms of ulceration, diseases of the testicles, and of the bones.

To all forms of constitutional syphilitic disease, the treatment by vapour is applicable, and beyond all doubt the most speedy, certain, and safe remedy that can be employed. Yet there are some forms of disease which yield with greater rapidity than others. That which gives way with the greatest difficulty is the induration which succeeds to the healing of a primary sore. Mr L. Parker means not that soft fulness which is sometimes found in such situations, but that cartilaginous hardness which is met with under the skin, and which is sure, sooner or later, to end in local or constitutional mischief. He has seen cases which have resisted all modes of treatment but the baths; to these they yield but slowly; but they do yield, and with certainty, after other plans of treatment have been followed for months without success, or with but partial amendment.

Mr Langston Parker gives the details of thirty-two cases of secondary and constitutional symptoms, all treated with good results by this method. For these cases readers are recommended to consult the work itself, which certainly contains strong evidence in favour of the merits and the advantages of the method over any other mode of obtaining the therapeutic effects of quicksilver in this disease.

6. *Phosphate of Lime as a Remedy in Pulmonary Consumption.* By S. KNEELAND Junior, M.D. (American Journal of the Medical Sciences, July 1852. No. xlvii., New Series, p. 117.)

Pulmonary Consumption, like all other incurable and fatal diseases, has had a great number of remedies proposed for its treatment, and to effect what the proposers call cure. The multiplicity of remedies acts as an inducement to the proposal of new agents; because, as all the former ones have failed, the proposer or proposers believe that the new one which they advocate must succeed. The last remedial agents, for which their authors claimed great, and in some sense unfailing efficacy, are naphtha, or the pyroxylic spirit, and cod-liver oil. It must be presumed that they have cured many phthisical patients and saved many lives. But the mortality from consumption in London, continues at the rate of 7027 annually, and for all England at rather more than seventy thousand persons.

Dr S. Kneeland of Boston, in the United States, proposes phosphate of lime for the treatment of phthisis, we know not whether it can be said the cure of the disease. Yet he is less confident than many of the proposers of new remedies. He says that he does not pretend that the administration of the phosphate of lime possesses any advantage in the treatment of consumption; but he thinks that there are chemical and physiological reasons why it should be beneficial in this disease; and he adds, that there is a singular coincidence between this remedy and the product which has been generaly considered as the effort of nature to cure by a chemical transformation of tubercle.

Both albumen and fibrin, he argues, the chief original components of the tissues, contain about two per cent. of phosphate of lime. Liquid albumen dissolves phosphate of lime; and fibrin when burned, presents it in the ashes. It is agreed that the deposition of tubercular matter is a perverted or abnormal form of the process of nutrition, so that albuminous tubercle is deposited in the tissues, instead of the latter being replaced by organizable fibrin. Phosphate of lime he thinks, may have an important influence on the form which tubercle may assume. The salt also, is said to exert a singular influence over the formation of cells.

Dr Kneeland then directs attention to the fact announced by Laennec, that vomicæ were cicatrised by their conversion into calcareous matter; and he appeals in support of the same doctrine to the authority of M. Roger, Valleix, and others, who regard this transformation as identical with the cure of consumption or tubercular vomicæ in the lungs.

The chemical analysis of crude unorganized tubercle is: animal matter, 98 parts in 100; muriate of soda, ·15; phosphate and carbonate of lime, 1·85; and traces of oxide of iron. In the concretions the chemical elements are the same as in tubercle, but their proportions are very different; while tubercle has 98 parts in 100 of animal matter, the concretions have only 5; the latter are principally made up of *phosphate of lime*, while tubercle has only two per cent. of it, like albumen and fibrin. This looks very much like a calcareous impregnation of tubercle, or a chemical transformation from an excessive deposit of phosphate of lime; and this without the softening and discharge of the tuberculous matter.

The cicatrices, seen often at the summit of the lungs, have been considered since the time of Laënnec, as efforts of nature to heal the excavations in the lungs, by depositing in excess the phosphate of lime necessary for the formation of the cartilage of which the cicatrices of the lungs are usually formed. At any rate, we find cicatrices of caverns at the summit of the lungs, after the discharge of the softened tubercle. The lining false membrane becomes more and more cartilaginous, the walls are brought into contact, forming a fibro-cartilaginous fistula or hollow cicatrix, which may or may not contain concretions of phosphate of lime; though, if permeable to the air through any bronchial tube (of which the openings into the cavities are usually obliterated), we should expect to find this salt, which is almost always present in the fluids of sinuses and fistulæ communicating externally. According to Louis, these cicatrices are a rare and happy termination of tubercular cavities.

Many cases are on record of individuals having recovered after having presented unequivocal signs of phthisis, and who, dying years afterwards of another disease, have presented these calcareous concretions at the summit of the lungs. In view of such cases, it was asked by Andral, many years since, "Is it not possible that, the calcareous phosphate having become predominant, the secretion of tubercle may be arrested?";—as it were, extinguished?

As almost all the cases of cicatrization and calcareous transformation have been found in old persons, in whom tubercles are rare and few in number, it may be objected, with Louis, that there is no proof that these few tubercles, of which we find the traces and alterations, were not developed at an advanced age, when the vital power of the organs is diminished, and when such deposits are common in other parts of the system; making phthisis, as far as these facts go, curable only at the age when it is least common and least to be feared. It may be answered, that these pulmonary concretions are different from the ossifications common in aged persons; they present no traces of a fibrous character, as do the latter; they also contain only five per cent. of animal matter, while the latter contain thirty-five per cent.

We find, then, phosphate of lime, so essential to vegetable life, playing a very important part in the production of animal-cells, and in this way influencing the amount of organization in tuberculous growths. We find it in the cicatrices of pulmonary caverns;—and especially we find it in the concretions, which are acknowledged to be transformed tubercle, nature's successful attempt at the cure of phthisis. We hence naturally inquire, cannot medical art transform a tubercle, and cicatrize a cavity?

The author further thinks that cod-liver oil produces its beneficial and curative effects, both in consumption and in rickets, by causing the deposition of phosphate of lime; a result which he thinks must be ascribed to the phosphorus which it contains.

Dr Kneeland accordingly draws the inference that phosphate of lime should be given as a remedy for consumption. He is not a little puzzled, however, in what form, and from what source, the phosphate should be supplied; whether it should be given as phosphate of lime, or along with cod-liver oil, or in some other mode and with another vehicle.

It seems to be overlooked by the ingenious proposer of this medicinal agent, that pure chemical remedies have scarcely in one instance answered thechemical indication, which they were given to fulfil; and that the animal body is a sort of laboratory which takes the whole of this business in its own hands. Phosphate of lime is given as food, in bread, in various grains; and the system takes from these supplies as much as it has occasion for, and no more. If phosphate of lime were the only thing wanted to cicatrize and consolidate vomicæ, this they could easily get. But before this is done with thirty or forty vomicæ, it is unnecessary to say where the patient is.

III.—OBSTETRICAL MEDICINE.

7. *On a New Method of employing Cauterization in treating Diseases of the Cervix Uteri.* By Robert Ellis, F.L.S., &c. London, 1852. Pp. 23.

For several years past, the practice of treating certain disorders of the *cervix* and *os uteri*, by means of cauterizing agents, actual or potential, has been gaining ground in this country to a considerable extent. This has been in some degree connected with the greater prevalence of this order of diseases than was at one time admitted, and with the belief, among many practitioners, that, in a large proportion of instances of abnormal discharges, these will be found, upon examination, to depend upon the presence of one or more ulcers in, or other morbid conditions of, the *cervix uteri.* To enter upon this field of controversy is by no means our intention. We have already allowed two of the principal authorities, Dr Bennett and Mr Whitehead, in former volumes, to make known their sentiments, and the results of their observation. Time and investigation, observation and experience, will undoubtedly enable the members of the profession to determine, to what extent ulcers exist in these parts, and in what proportion they are causes of the discharges of which they have been very nearly uniformly alleged to be the cause. Not a few respectable practitioners still entertain serious, and, according to their belief, well-founded doubts, whether these ulcers are so frequently the cause of the discharges. But, as has been already said, it is unnecessary at present to enter into this controversy.

The present author seems quite aware of the danger of opening up this question, though it is in some degree connected with the use of the remedy, and the form in which it is to be applied, which he has proposed.

"This is not the place to discuss the nature of the diseases affecting the *cervix uteri*, some forms of which have, perhaps in some instances incorrectly, received the name of Ulceration. And in the following pages I have carefully avoided the indiscriminate use of this term. That an extensively diseased, granular, and pus-secreting condition of the mucous lining within and around the *cervix uteri* has existed in all the cases I have treated, and that this has been accompanied by various phases of induration and hypertrophy of the cervix itself, with great debility and extensive disturbance of the general health, is certain; nor less so, the fact that a healthy local condition, and a corresponding constitutional improvement, have resulted from the treatment (almost exclusively local), I have adopted."

Whether, therefore, it be ulceration, or merely chronic inflammation, the granular suppurating state of the *os* and *cervix uteri*, the great remedy has been supposed to consist in the use of caustic agents or escharotics.

It is scarcely necessary to say that there are two forms of cauterizing or escharotic agents ;—one, the actual, that is, the hot iron ; the other, the potential or chemical, that is, the application of various chemical substances which act on the animal tissues, so as to kill more or less of the surface touched. Of the latter substances, the most common are the caustic potass, that is, the *potassa fusa*, the lime and potass caustic, the mineral acids, solid nitrate of silver and nitrate of mercury, butter of antimony, namely, chloride of antimony, and one or two more.

Among these applications, the solid nitrate of silver, and astringent substances, form the slighter kinds of local dressing ; the acid nitrate of mercury, the Vienna paste, *potassa fusa*, and the actual cautery the more severe. The common practice in England is at this moment to use the solid nitrate of silver in all slighter cases, the mineral acids and the acid nitrate of mercury in more severe forms, and the *potassa cum calce** in very obstinate cases. The actual cautery appears to have been scarcely used in English practice.

The remedy to which Mr Ellis assigns the preference, is the actual or metallic cautery applied in a peculiar manner by means of electricity.

The surgical value of the actual cautery has been long known, though, in our own country at least, it has been little used. Dr Bennett, in his work on Diseases of the Uterus, gives a good summary of the facts generally known relating to the employment of the actual cautery as a means of treatment of what he denominates ulceration or inflammatory induration of the neck of the uterus. Celsus, he observes, recommends ulcers of the prolapsed uterus to be cauterised with the actual cautery, and some modern surgeons have proposed the same means of treatment, as, for instance, Percy and Baron Larry. It does not, however, appear that their suggestions were ever acted upon until adopted by M. Jobert de Lamballe, the surgeon to the Hôpital St Louis, Paris, who has for many years resorted with great success to this mode of treating diseases of the neck of the uterus.

The manner in which M. Jobert applies the cautery is by employing a conical ivory speculum as a protection from the radiant heat of the instrument, ivory being a bad conductor of caloric. One, two, or three olive-shaped cauteries, heated to whiteness, are then extinguished on the part of the cervix which has to be cauterized. But little pain is experienced by the patient either at the time or subsequently,—the eschar falling from the ninth to the tenth day according to its depth. M. Jobert believes that he has obtained better and more satisfactory results under this course of treatment than by the employment of the more powerful chemical escharotics, which, Mr Ellis thinks are open to some serious objections.

As may be readily conceived, the application of the actual cautery in the manner above described, would naturally excite great alarm and fearful anticipation in the mind of the patient. The presence of the furnace for heating the instruments, and the inevitable confusion and bustle attendant upon their rapid removal from it, created a most undesirable degree of apprehension, although the actual pain experienced was probably not more than that of an ordinary dressing with the nitrate of silver. In private practice, it appears almost impossible to adopt this method, in the absence of the facilities afforded to the hospital surgeon,

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* For a very simple and ingenious method of using this powerful but somewhat dangerous substance, the profession is indebted to Dr J. H. Bennett.

It is therefore easy to understand, that, however beneficial in its influence over the disease, other means would be, and have been sought; so that the use of actual cautery in diseases of the cervix has been chiefly confined to the practice of the surgeon in question. If we add to all these the risk of a misdirection of the instrument in the hurry of seeking to apply it while yet glowing to the diseased surface, enough will have been said to indicate its unsuitability for general practice, however successful in the skilful hands of M. Jobert.

Mr Ellis is convinced of the value of cauterization by caloric in almost every form of disease of long standing of the *cervix uteri*, if a means could be adopted to which the above objections do not apply. What is required is an instrument capable of being heated to whiteness without a furnace, without fire in short, which could be applied even in a cold state, the heat being developed at the will of the operator, being instantly under his control, and the instrument being of the ordinary dimension of the caustic holder. The fulfilment of all these conditions may seem at first sight impossible; yet Mr Ellis is prepared to shew not only that it is possible, but that he has extensively and with success used such an arrangement.

For some time before he could reduce the idea to a practical form, he thought that in the incandescence of a heated wire, by the resistance it offers to the electro-galvanic current, a most valuable aid to the surgeon was promised; and it is known on the Continent, and partially also in England, a wire thus heated has been used as a substitute for the knife in the division of soft parts, and in the cure of fistulous sinuses.* Having several cases of obstinate disease of the *cervix uteri* at that period under treatment, he determined to apply a wire thus heated to small parts of the diseased surface and to observe its effects. For this purpose, Mr Ellis obtained two small lengths of wire insulated by a coating of gutta-percha, which he twisted together so as to form a rod, leaving the ends free and bifurcated. The lower ends were then connected with a good galvanic battery, and a twisted platina wire connected the upper ends, which were to be applied to the diseased cervix. On completing the circuit, the platina wire was instantly heated to whiteness, and, on breaking the connection with the battery, it was as quickly cooled again. Such was the form of instrument first adopted. On practically applying it to the cases in question, Mr Ellis became fully convinced of the value of the principle, however imperfect the means, and in a short time he was equally satisfied as to its influence in hastening forward the progress to cicatrization of some very intractable forms of disease. The minute surface touched by the heated wire rendered it necessary to apply it several times in succession over even a small area of disease, and it became evident, that, for any practical value, it was necessary to increase the heated surface, and also to prolong slightly its intensity of action, since the wire was instantly quenched on touching the granulations of the sore. It was also necessary to have the heat so thoroughly under control, that, by the simple pressure of the finger, it would be turned on or off at will. He proposes the following simple instrument, which may be made without difficulty at home, as one that will be found to answer every purpose for the cauterization of disease of the neck of the womb. At least it has been constantly used by him in many cases, and, as yet, he has seen little occasion to desire a better.

The instrument consists essentially of two conductors of the current electricity, the lower ends of which are connected with a battery, and the upper are united by a platina wire coiled around a porcelain tube. A good sized silver catheter, straightened out and the end cut off, forms the body of his instrument. At the upper end it was slit open and broached, so as to form a socket for the porcelain cauterizer, and also to admit the internal wire to pass out. Within the silver tube a piece of stout copper wire, clothed with an insulating covering,—an old elastic catheter answers every purpose,—is contained, and appears at the upper extremity, where the wire is left bare for about half an inch for the attachment of the platina wire. On connecting the lower end of this wire with one wire of the battery, and the surface of the silver tube with the other, we have a very firm, simple, aud compact arrangement of the two poles. At the upper end, where the heat is to be developed, these poles are separated by a tube of porcelain, a little thicker than the ordinary stick of nitrate of silver. The object of this tube is to support the coil of platina wire intervening between the two extremities of the poles, that is to say, between the end of the silver tube and that of its contained copper wire. But it fulfils a far more important purpose;—it serves as a reservoir of heat, and gives, in short, to the instrument all its novelty and value in a surgical sense. About a foot of thin platina wire is made into a coil, and the one end being firmly connected to the internal copper wire, and the other to the upper end of the silver tube, the circuit is thus completed, and the electric current, in passing from the copper wire to the silver tube, heats to incandescence the platina wire through which the connection is made. As this is coiled round the tube of porcelain,* its heat is imparted to that, and a body of intensely heated material is thus obtained over an extent of about three-quarters of an inch in length. Immediately that the circuit is broken by removing the wire from the battery from its contact with the surface of the silver tube, the heat disappears, reappearing instantly on making contact again.

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* Mr Ellis refers to the publication of Mr Marshall on the uses of electric heat in surgery.

With the description above given, it may be hoped the reader will find no difficulty in constructing a similar arrangement for his own use. It would not be difficult to give this instrument a more complicated aspect, and perhaps, in some respects, to increase the convenience of its use, by a somewhat different arrangement of the battery wires, and of the means for making and breaking contact. But in the simple form above described, its efficiency is complete, and the advantages of simplicity and economy of construction are not sacrificed. Should any medical man find any difficulty in obtaining such an apparatus, and feel desirous of having an instrument made after the model of his own, Mr Ellis is ready to place it in the hands of any competent surgical instrument maker for that purpose.

The battery power generally employed is a good Grove's battery of five cells, which, if in good working order, is generally sufficient alone, and a cast-iron, or Maynooth battery, of four cells. For every purpose, two Grove's batteries, of four cells each, give a sufficient intensity of current electricity to heat to whiteness the cautery recommended. It is very desirable to have a full supply of battery power; the cautery should always be white hot to be of practical value. The conducting wires may be of copper wire-rope, which is very flexible, and thus easily adapted to the position of the patient; but they should be sufficiently thick to offer no resistance to the passage of the electric current, otherwise a portion of the power of the batteries is absolutely lost. In order to obviate the annoyance of the nitrous fumes evolved, and not less importantly the apprehension found to be occasioned by the sight of two batteries in work,

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* A piece of the stem of a common tobacco-pipe, about an inch and a half in length, answers very well, and is of a convenient size for general use.

Mr Ellis generally arranges them outside a window, and altogether out of sight, the wires alone entering the apartment where he operates.

The cylindrical form of cautery is perhaps the most universally useful; but when necessary, small olive-shaped pieces of porcelain may be used, which can be made to order at the potter's. Such olives should have a cylindrical stem, to fit into the socket formed by the upper part of the silver tube.

The pain of this operation is generally less than that of applying the solid nitrate of silver to the *cervix uteri*. This will not appear surprising to those who are familiar with the application of the electrically-heated wire to the relief of toothache. The instantaneous operation of the cautery appears to destroy the sensation of the parts to which it is applied. If a lower degree of heat were used, it might then be somewhat more painful. In the great majority of instances in which the platinum cautery has been used, the persons have said they felt little or no pain, and they have generally considered the simple operation of dressing with nitrate of silver a more serious one, in regard of the sensations experienced, than the application of the actual cautery. So little apprehension has arisen in the minds of those, on whom this instrument has been used, that in no instance where it might have been proposed, have they expressed a fear of the repetition of its use.

It is necessary to bear in mind, that however painless the operation may comparatively appear, it is nevertheless one which requires prudence and discretion in its performance; for the agency employed is one of intense activity and power. The heat of the cautery above described is different from that of an ordinary cautery in this important respect, that it is a *self-maintained* heat, and is consequently capable of producing a far deeper eschar than that of the common cautery. This cautery is also distinguished by the advantage of being capable of instant ignition and of extinction at the will of the operator. Its use must consequently be under the guidance of a skilled experience, a remark, applicable in equal proportion to every other operation on these organs. Yet less risk is actually incurred in its employment than in that of the *potassa fusa* and other violent chemical escharotics.

Another advantage of this cautery is the precise limit capable of being assigned to its effects. If the *potassa fusa* run over the edge of the *cervix uteri* on to the vaginal walls, it may cause very serious mischief, and, at the least, troublesome sores are produced in perfectly healthy parts. Such is the case even when the milder acid-nitrate of mercury is employed. There is a constant risk of extension of injury to healthy parts, and, although in skilful hands, this may be greatly or wholly obviated, the risk still remains. From this disadvantage, the electro-platinum cautery is entirely free. With a good supply of battery power, it might even be laid *cold* on the diseased surface, and instantly heated to the necessary degree at the operator's discretion. There is no risk of directly affecting other parts than that desired to be destroyed, and the depth of the eschar is absolutely determinable by the prolongation or otherwise of the application, or of the contact of the conducting wire with the surface of the silver tube. The small size of the instrument,—the instrument used by him not being thicker in any part than a quill of small diameter,—is also a great advantage, since even when a very small speculum is used, it may be guided by the eye precisely to the seat of mischief.

The following is the method adopted by Mr Ellis in its application. The patient, in ordinary cases, is placed in the usual obstetric position, on the left side, in a good light (daylight). This he believes to be the best position for this, and indeed for almost all the common operations on the *cervix uteri;* and, by a little tact, a very complete view of the disease can be gained without unnecessary exposure of the person or removal of the apparel. A cylindrical speculum of glass, protected by caoutchouc or gutta percha, is then introduced, and the diseased surface is fully placed in the field of the instrument. This may almost always be accomplished by rolling the speculum round, when the indurated cervix quickly protrudes into its upper part. The glass being a bad conductor, does not permit the heat to be felt by the patient; and as the whole operation is very quickly done, there is not the smallest risk of its fracture. Mr Ellis has used the same instrument constantly without injury to it. But if a fear were entertained as to its use, then an ivory speculum may be used, or the ordinary bivalve instrument, the lateral openings being protected by a slip of metal. Contact is then made between the wires, and the porcelain is almost instantly heated to the fit temperature. In this state it should be steadily and quickly introduced through the speculum and laid on the diseased surface for a second or two; or, if a deeper eschar is required, for about three or four seconds; the contact is then broken, and the instrument withdrawn. Sometimes there may be a little hæmorrhage, but this always yields to the application of the solid nitrate of silver. As a general rule, Mr Ellis applies the nitrate of silver after the application of the cautery, since it is possible that some portion of the diseased surface, either externally or within the cervix, may have escaped its action. The patient is then directed to keep in bed for two or three days, and to preserve a recumbent posture as far as possible for a week or ten days, until the separation of the eschar.

After the operation, for two or three days pain may be felt, and there may be some local tenderness. There is often, also, a coloured discharge. This, however, is certainly not unsalutary when confined within due limits, relieving, as it does, the congested condition of the bloodvessels in the diseased organ. In about a week, the patient recovers her usual condition, and from that time the improvement in her state is pretty constant. The after treatment consists simply in the application, occasionally, of the nitrate of silver for a few weeks, until complete cicatrization has taken place. The patient not unfrequently considers herself to be worse on the separation of the eschar, and the purulent discharge following that process appears to confirm this supposition. In a little while, however, this discharge diminishes and soon ceases wholly, and the corresponding constitutional amendment is proportionably rapid.

With regard to the time occupied in the cure, Mr Ellis states, as a general fact, that cases can be cured much more rapidly by this method, if it is skilfully and intelligently used, than by the ordinary means.

In the after treatment great importance is attached to the employment of injections. Among the poor, this is a point to which much attention ought to be directed, for the price of any instrument of efficiency is often beyond their means. For such cases he recommends a very simple and economical instrument. It consists of a piece of gutta percha tube, half an inch in diameter and six feet in length, the lower end of which is curved upwards. To the upper end is attached a small piece of vulcanized caoutchouc tube, and when the instrument is to be used, this is slipped over the mouth of a tea-kettle into which the injection (or cold water) is poured. On the latter being placed on the shelf, and the other end being introduced to the seat of disease, a constant and pretty forcible stream may be obtained by tilting the kettle forward. A syphon of the same kind has also been recommended, but it is less easily understood and used than the simple tube above described.

From the progress made by many patients after the application of the electrical cautery, Mr Ellis is disposed to believe, that, in many instances, the cure might proceed from that time without further remedial assistance. But the time occupied would be undoubtedly longer than if the nitrate of silver were steadily applied, so as to preserve a healthy character in the granulations of the sore.

Mr Ellis has applied this method of treatment to a very considerable number of undoubted cases of disease of the *cervix uteri*, and he affirms that in no single instance has it ever been followed by any bad results. On the contrary, intractable disease has rapidly assumed a healthy character, induration and even inflammatory hypertrophy have gradually disappeared, and an amount of progressive and rapid amendment has taken place, which it had been impossible to secure with the most diligent employment of ordinary escharotics.

IV.—TOXICOLOGY.

8. *Instance of Poisoning from the External Application of* Cocculus indicus; (Menispermum Cocculus; *Moonseed*). Reported by WILLIAM B. THOMPSON, Sen. House Surgeon, Emigrants' Hospital, Ward's Island.

Bridget Maddon, aged six, a native of Ireland, blue eyes and fair complexion, was admitted into the surgical division, accompanied by a sister and brother, on the evening of February 3d, 1852, for porrigo of the scalp, accompanied with vermin.

The nurse was directed, after cutting the hair close, to wash their heads with an infusion of Delphinia (the house prescription). Not having the article at the time, the apothecary sent an infusion of *Cocculus indicus* (used in the hospital for the same purpose), made by displacement,—a pound to three gallons of dilute alcohol,—six ounces of which was used for the three children.

Half an hour after its application Mr Thompson was sent for to see Bridget, whom he found labouring under tetanic spasms with the pupils contracted to their smallest diameter. As the spasm abated, the pupils became dilated to their fullest extent, and again contracting as the spasm returned. By touching the eyelids, the spasm could be produced at pleasure; this was repeated until all present became fully satisfied the difficulty was seated in the excito-motory system, and was the result of irritation rather than inflammation. The case was treated as one of idiopathic tetanus, with counter-irritation, warm baths, and antispasmodics; but the attacks continued to increase in frequency and force, until the patient sank about midnight of the same day. The body was examined anatomically thirty hours after death. The viscera, brain, and its appendages, were minutely examined, and all found to be in a healthy condition.

The nature of the case is confirmed by the younger sister, aged four, being attacked in the same manner and exhibiting the same symptoms as she (Bridget) did. Being in the ward at the time, a warm bath was ordered, into which she was placed as soon as the first spasm was over; a mustard plaster was applied over the abdomen, and to the legs and feet as high as the knee; injections of the tincture of assafœtida were thrown into the rectum, and a few drops administered by the mouth every hour. By persevering with these means the spasms subsided gradually after an elapse of three hours from the commencement of the attack. On the morning of the 4th inst. the patient's body and arms were again covered with an eruption resembling scarlatina, which gradually faded away during the day.

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